With the author's compliments.

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Croonian Lectures

ON

THE OBJECTIVE STUDY OF NEUROSIS.

Delivered on June 9th, 14th, 16th, 21st, before the Royal College of Physicians of London.

By F. L. Golla, M.B. Oxon., F.R.C.P. Lond.,
PHYSICIAN TO ST. GEORGE'S HOSPITAL.

LECTURE I.

In this series of lectures I propose to deal with certain types of movement that are the manifestations of those phases of cerebral activity which have been generally considered to be confined to molecular changes in the cerebral cortex and its great ganglia. I refer to processes of thought and feeling. The modern knowledge of the functions of the brain has led us to look to this organ almost exclusively for the manifestations of mental activity; we are apt to forget that to such a great psychologist as Aristotle the brain was nothing but a mass of cold matter whose function was to cool the blood coming from the heart and lungs, serving only as an intermediary link between the organs of sight, smell, and hearing, and the heart where their sensations were perceived. It is through our concentration on the cerebral aspect of mental activity that we have been so slow to realise that the body partakes in the movements that express this activity and, by ignoring these bodily signs, the possibility of establishing an objective standard in our interpretation of neurotic disturbance is, to a great extent, lost. What, then, are data furnished by objective observation of the activity of the nervous system? I cannot do better than quote the views of Bergson as summarised by Hoffding: "All that takes place in the surrounding world and in my own body, including my brain and nervous system, consists only of movements of different kinds and degrees. From the external world movement is spread abroad through our body. It reaches the spinal column and the brain by way of the sense organs and the nerves, and proceeds into peripheral movements. Our body is an instrument which receives movement from the outside, and restores it to the external world. There is not from this point of view any difference between the brain and other parts of the body. The brain is merely stronger than the other organs in preserving the action that it has thus received, and does not always reproduce

them in movement immediately but often makes them coöperate in ulterior movements. The body, including the brain, is an instrument of movement and nothing else. To no degree, in no sense, from no point of view, does it serve to prepare and still less to explain a representation. That which, in our perceptions, can be explained by the action of the brain, includes the actions which are commenced or prepared or occasioned, but not our perceptions themselves. Still less does the activity of the brain comprise any explanation of memory and of higher mental activity. If everything in the brain and outside is movement we must not look for anything else than what is observable. The nervous system has only physical properties, and has no other power than that of receiving, preserving, and continuing movement. Movement alone is sensible to us and movement can produce nothing but movement. The whole effect of physical processes is exhausted by the effort of motive adaptation (le travail d'adaptation motrice). If the brain is ill, it is only movements and nothing else that are arrested." No scientific worker is likely to quarrel with this view, but it is important to realise that its adoption leaves us untrammelled by any metaphysical implications ; it is as consistent with materialism as with idealism, with monism as with dualism.

Relation of Psychology to Physiology.

It will be plain that a psychology founded on this objective basis leaves untouched all that constitutes for us the chief interest in life. In terms of objective psychology such expressions as meaning and purpose can find no place ; by severing it from teleology, history, from the point of view of the psychologist, will become unintelligible. This was, indeed, the view of psychology held by Münsterberg, who argued that the sole function of psychology is to provide us with mechanical uniformities of sequence by the aid whereof to calculate the future behaviour of our fellows in so far as it is not modified by fresh purposive initiative, and that the whole of psychology is a temporary stop-gap, by which we eke out our defective physiology, but which must sooner or later cease to be of use, and therefore cease to exist, as physiology advances. Indeed, to such a pitch must psychology necessarily be brought, if we listen to the warnings of Avernarius on the besetting sin of "introjection."

Now this may or may not be a desirable state of affairs for the psychologist, but I maintain that for us as physicians it is the only possible. The late Dr. Mercier entitled his text-book of insanity " Disorders of Conduct," for of mind he said he knew nothing. It may, indeed, be held with some plausibility that there are no such things as disorders of mind. As physicians we are confronted with cases exhibiting disorders of conduct—that is, of the mechanism of expression, and our psychology must stand in exactly the same relation to mechanism as does our physical science. It is necessary to insist on these

methodological points because we find both neurology and, to a lesser degree, physiology in a state of great confusion owing to their non-observance. Consider the mess that neurology has made of the subject of aphasia, by a careless use of physiological and metaphysical concepts. It arose merely because neurologists talked of memory—a non-physical entity —being stored up in nerve cells without realising that they were talking nonsense. All that they were really entitled to say is that the mechanism of representation is preserved in certain neurones.

Physical Disturbances the Primary Factor in the Neuroses.

Thanks to the labours of Pierre Marie, and in more recent times to those of Head, we are slowly reconstituting our knowledge of aphasia as being in all its forms a disorder of the mechanisms of expression. It is interesting to note that we have only now arrived at the same conclusion on the subject of aphasia as that advanced by a philosopher, M. Bergson, nearly 30 years ago, as soon as he detected the confusion of thought that lay at the root of our difficulties. It is from much the same causes that so much confusion arises in physiological literature on the subject of sensation—that is, from the incautious use of non-physical terms in the description of physical states. The point of view that I am about to deal with in these lectures has for its object an attempt to define in objective terms the chief data that may lead us ultimately to formulate the nature of the physical disturbance of mechanism that constitutes the primary factor in the neuroses. That such a physical disturbance is the cause of the neuropathic condition and precedes its manifestation I hold very strongly. One fact emerges from all the investigations that have been conducted in recent years by the methods of analytical psychology, and that is that in the vast majority of cases the experiences of the neurotic differ in no way from those that fall to the lot of ordinary healthy men. The data furnished by the war are often cited as an argument to the contrary, but I think that they really furnish irrefutable evidence that an organic disturbance or failure of organic equilibrium preceded the manifestation of the neurotic symptoms and could not have been caused, however much it may have been aggravated, by the individual experiences. In the armies of the Allies and those of the enemy millions of men were exposed to similar conditions, yet only a small number were brought to our hospitals suffering from a neurosis. Doubtless all of us who were in the line showed for a longer or shorter period the disturbances due to fatigue or functional hypertrophy of certain nervous mechanisms, but only those succumbed who were organically unsound. Sir Frederick Mott was able to obtain a history of pre-existing neurotic symptoms in 80 per cent. of the cases of soldiers under his care for neurotic disorders. But the point hardly needs pressing; had those who have contributed to the literature of the war neuroses

been permitted to undertake regimental duty in the line, thus to know their men, as one only can do by constant contact, they would have found little difficulty in spotting the future cases of neurosis before ever a shot was fired. Were these cases of organic nervous weakness the victims of infantile and childhood experiences that left an indelible mark on their nervous constitution? There is no reason to suppose that as a whole their experiences differed from those of the average child of their particular social milieu. We are, I think, justified in assuming that an organic disability exists as an antecedent to every neurosis, and in employing methods for objective evaluation of organic efficiency in looking for it.

Study of Conduct and of Specific Reaction or Movement.

Such an objective study must necessarily be conducted along many lines of observation; but, broadly speaking, there are two methods at our disposal—the study of conduct and the study of specific bodily reactions or movements.

The study of conduct is a purely objective study of all those activities which collectively represent to us all that we can know by observation of the exterior aspect of life. The study of movements is supplementary to the study of conduct. By movement we understand not only the manifest muscular reactions, but also secretory and circulatory responses. The study of conduct in a search for signs pointing to definite organic disorders must of necessity furnish much evidence that is ambiguous and much that is definitely misleading. The recognition of the actuality of our own and our fellow men's life of unique and incommunicable feeling compels us to admit the existence of much that from its incommunicable nature falls outside the sphere of physical reality. The weakness of any objective study of conduct is that much of our information depends on verbal communications from the subject studied.

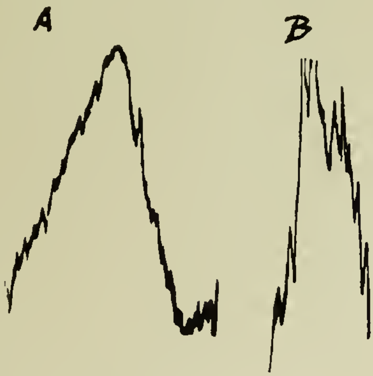
Now, quite apart from this unique incommunicable aspect of experience, there are a host of affective states and sensory experiences that we have to a great extent in common and that we find the greatest difficulty in communicating. Language is in its essence a method of communication evolved solely in response to practical needs—that is, for the communication of information essential to the maintenance of our social life. The communication of the greater part of our experience serves no such useful purpose, and hence has never acted as a stimulus for the evolution of appropriate language. Those of us who are musicians know that we have a whole range of experience which in its broader aspects we have no reason to suppose to be unique, but which for all that remains practically incommunicable. It is the function of the artist to bridge this gap in our social life by presenting his immediate intuition of reality, but we all know how rarely and how imperfectly art succeeds. It is precisely for this reason that the study of those

movements which are the exterior signs of states of feeling becomes so important. When our methods of observation become sufficiently perfected we may be able to check by means of these stereotyped forms of reaction the deductions that we draw from the concerted actions, words, and silences of our patients.

Types of Movement Studied.

The movements which I propose to study are of two types—those which are the exterior manifestations of the specialised response of certain nervous mechanisms, and those which connote the generalised reaction of the body as a whole to stimuli which disturb or threaten the unity of vital action. With movements of the first type we may deal briefly, because their disturbances belong rather to the category of what are known as organic nervous diseases than to that of the neuroses. When we speak and at the same time press the hand on the throat we become aware of movements of the larynx which are easily palpable. They are still more pronounced when we sing, and by pressing the nail of the index finger on the free edge of the upper border of the thyroid cartilage we may readily feel the cartilage move upwards higher and higher with each ascending note of an octave and descend by similar steps as we go down the scale. These movements may be recorded if we fix to the neck a firm leather collar supporting a small spring, one end of which presses downwards on the cartilage and the other is fixed to the collar. At right angles from the spring a small button is soldered which impinges on the drum of an air capsule conveniently attached to the collar. The vibrations of the tambour membrane are conveyed by a rubber tube to another tambour fixed on a stand and this in turn actuates an optical lever on which a beam of light is projected. The magnified excursions of the optical lever are recorded photographically. Fig. 1, A, is a record of the movements of the cartilage in singing up and down an octave. Now, if instead of singing audibly I think of the notes of the octave, and at the same time record the laryngeal movements, I shall obtain a similar picture (Fig. 1, B). The movements are generally smaller than in actual singing, and for purposes of comparison they have been roughly magnified three times in this figure by moving the recording cylinder further away from the optical lever.

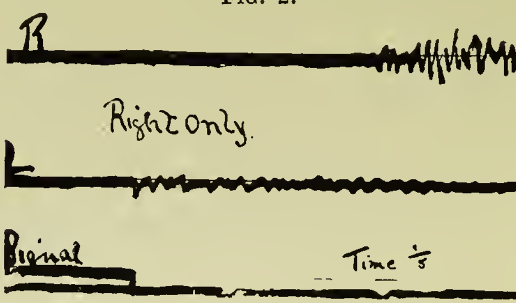
Fig. 1.



Thought a Mode of Movement, whether Vocalised or Silent.

Thought is expressed either by external or internal speeeh, and whether the speech be externalised or internal it is a mode of movement. If I reeite to myself a speeeh or a poem I ean demonstrate the oeeurrence of similar laryngeal movements, though in this ease their relation to the eorresponding speeeh movements eannot be demonstrated with the same ease. Now to think of the notes of an oetave or to recite a poem silently is not quite the same thing as to sit quietly and think over any problem or make arrangements for the future. In the first ease we are trying to represent to ourselves every tonal step of the oetave and every word of the poem, whereas in the latter we are not so eoneerned with the verbal niceties of our problem as with the eonelusions to whieh it leads. Our motor manifestations in the seeond ease will be more sketehy, the hint of the beginning of a word movement will be suffieient to eonvey to us the sensory impression of the word just as in reading psyehologieal experiments have shown that the eye does not move along the line of print from

Fig. 2.



letter to letter ; but that in a series of jerky movements, it seizes suffieient letters to allow of the reeognition of any one word and then darts on to the next. The motor manifestations eonstitute what Ribot has happily termed the skeleton of our presentations. What, then, fills in the skeleton outlines ? The answer varies for eaeh individual. Ask a number of men to think of a given multiplieation sum—some of them will see the figures, others will hear them, others will eombine a feeble visualisation with a eertain sense of movement, feeling as if the eye were travelling down a row of figures and eaeh were visualised in turn in its plaee. Those who manifest most markedly laryngeal movement probably fill in the gaps of the skeleton with kinæsthetie representations of the voeal movements that do not aetualise. The rôle of aetual speeeh movements in representation is probably under-estimated. Had I been asked some years ago before I had eonsidered the question in what way I represented to myself the oetave I should have answered unhesitatingly that I heard it, I have

now little doubt but that my laryngeal movements are the first link in evoking the ultimate auditory representation, because though I am told that I have a fairly good ear, my musical memory is to a great extent limited to the representation of those notes that lie in the compass of my voice. Similarly, in visual representation our visual images appear to be to a great extent stimulated by kinæsthetic images arising from the centres for movements of the eye muscles and in extreme cases the stimulation of the motor cell may translate itself into actual movement. I have often performed the experiment of asking an unsophisticated subject to describe to me Trafalgar Square as he would see it if he stood at the base of the Charles I.'s monument. If one is careful to avoid standing exactly face to face, and thus allowing him to fix his eyes on the observer's, in nine cases out of ten I find that as he reels off the series of objects to be seen his eyes move from the right when he mentions Morley's Hotel, to the left when he comes to the Union Club, and upwards as he describes the Nelson monument. It is not only in the mechanism of internal speech that we find these movements, but in the preparation for action. Figs. 3 and 4 represent the results I obtained in some experiments in which the subject was asked to dorsiflex both hands the moment he heard a sound signal. Both forearms were lying with the palms downwards on the table and the extensor group of each forearm was connected to two galvanometers which were constituted by two of the loops in an oscillograph. In the case illustrated the patient was a left-handed man, and in the experiment recorded in Fig. 2 he was asked to raise the right hand only when the signal sounded. It will be observed that from the time that the signal sounded till the actual moment that the right hand was raised there was a well-marked electrical vibration observable in the extensor group of the left arm. The idea of movement in this subject was represented by an actual activation of the muscles, and as he thought of all his movements primarily in terms of the left hand, it was the left extensors that responded. When he was asked to dorsiflex both hands simultaneously, we again see the representation of movement in the left hand (Fig. 3). On asking him to move the left hand alone, the same left-sided representation occurred (Fig. 4). It appears that this connoted the idea of movement on which he was concentrating, and was not in any way a preparatory performance, since the latent times of moving the left hand alone and the right hand alone were identical. In a right-handed subject the phenomenon was reversed. It is of interest to note that for reasons that I shall deal with when we come to talk of tonus one can be sure that this motor representation was of the nature of a voluntary muscular contraction, not an increase of muscle tonus. This motor representation has its influence on all our forms of thought. A very cursory survey of the expressions that we use, whether in the language of description or of abstract reasoning, is enough to show that a dynamic symbolism is universal.

Some time ago Binet made some observations on chess players who were able to play several games blindfold and simultaneously. We should anticipate that such men had an abnormally acute power of visual representation; but though they all stated that they could without much difficulty visualise the chess-board, they agreed that in actual play they never did so, and that visualisation would, if anything,

FIG. 3.

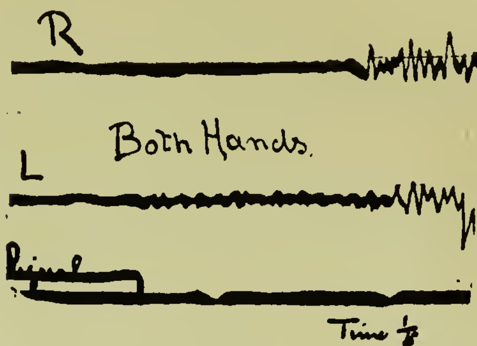
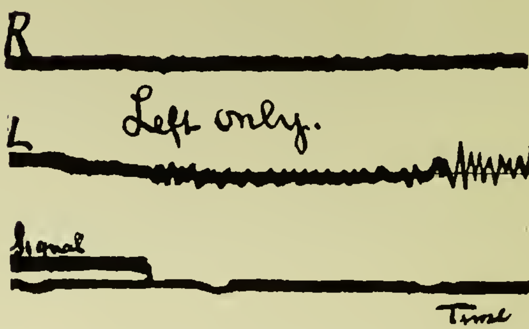


FIG. 4.



rather impede than help them. The chessboard to them was represented in dynamic terms as a congeries of contending forces. A knight was a force acting diagonally and a rook vertically. I do not intend to pursue the subject of these specific forms of motor reactions further, because, as I said before, their disturbances belong more appropriately to the study of gross organic lesions; but it must not be assumed that the specific mechanism by which representations normally occur is without influence on the symptom-atology of the organic disturbance which, I hope to show, underlies the neuroses. As to the nature of this influence on conduct I will confine myself to one

hint. A surgeon with world-wide experience of the congenitally blind once told me that when a blind man was vicious his abnormality often assumed forms more gross and detestable than occur among the sane with normal eyesight. I have the impression that men with a predominantly auditory type of representation are less nice about physical matters than those with a predominantly visual type, and that the visualist is less nice about the choice of words and less prone to take umbrage at a harsh expression than is the auditive.

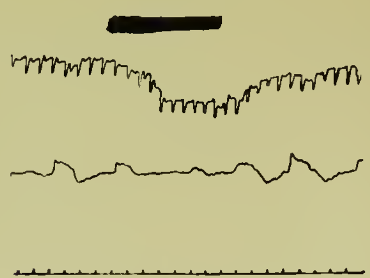
Connexion of Muscular Tension with Intellectual or Physical Effort.

I next propose to deal with those movements that connote a generalised reaction of the organism, from the study of which I think we can gain much information as to the fundamental disturbances of mechanism in the neuroses. In the first place I will invite you to study those organic reactions by which the body responds as a whole to stimuli occasioned by the vigorous activity of some specific mechanism. To denote this form of response I prefer to avoid any terms that have subjective implications, such as attention and conation. I shall therefore speak of this type of reaction as effort, and distinguish for purely methodological reasons intellectual and physical effort.

That effort is accompanied by some general alteration of muscular tension has been generally appreciated in a vague fashion, but I have found no record of any attempt to measure and determine the nature of these alterations. Now, there is a very simple and homely method by which the alterations of tension in the quadriceps muscle can be verified. Everyone has at some time or other noticed that when the legs are crossed in such fashion as to allow the crossing leg to hang almost freely downwards the foot can be observed to pulsate up and down with every beat of the pulse; the lower leg, in fact, constitutes the lever of a sphygmograph. If when the legs are so adjusted as to show the pulsation and we are reclining comfortably in an armchair in front of a tiled fireplace we carefully observe the height of the pulsating toecap against the line of intersection of the tiles, we can readily verify any change in the level of the foot. Now, still keeping the eye on the alignment between the toecap and the crevice in the tiles, make some voluntary effort—either attempt to perform some difficult piece of mental arithmetic or grip some object with all your force. It will be seen that coincidently with the effort there has been an appreciable upward movement of the foot—that is, a shortening of the extensor muscles of the thigh has occurred. It is quite easy to measure this shortening and to determine its nature. To do so, it is above all necessary to ensure that the crossing leg is completely relaxed and that the lower extremity is hanging sufficiently freely to exert a constant pull on the quadriceps tendon. This state is present

when the foot can be observed to be pulsating vigorously with each heart beat, and is brought about by taking care that the free suspension is in no wise interfered with by the underlying knee supporting the calf muscles. The knee of the crossed leg should lie in the upper part of the popliteal space of the crossing leg. The toecap of the shoe is then attached by a thread to a suitably placed lever whose excursions are recorded on the kymograph. A shortening of the quadriceps and upward movement of the foot will be recorded by a downward movement of the lever. Fig. 5 is a record of the quadriceps shortening manifested in one type of effort, the solving of a problem

Fig. 5.

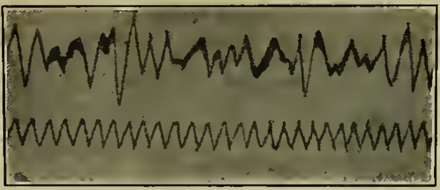


The upper tracing is that of the movements of the foot. A downward movement of the lever indicates an upward movement of the foot. The lower tracing is that of the respiration. Time in seconds. Thick black line indicates approximate duration of stimulus—a multiplication.

in mental arithmetic. It is now necessary to inquire into the nature of this contraction of the quadriceps; is it of the nature of a so-called voluntary contraction —that is to say, produced by direct cortical innervation, or is it due to an increase of postural tonus? The work of Wertheim Salomonson has shown that it is possible to differentiate between these two types of muscular contraction. It was shown by Piper that the electromyogram of a voluntary contraction as recorded by the string galvanometer is manifested by an irregular oscillation having the frequency of roughly 50 vibrations per second. These vibrations were considered by Piper to indicate the number of motor nerve impulses reaching the muscle in a second. It is for many reasons certain that they really indicate nothing of the sort, but they served Salomonson to differentiate a voluntary muscular contraction from a tonic contraction which he proved to be entirely innocent of these vibrations. Fig. 6 represents a record taken by the Bock Thoma oscillograph of the vibrations occurring during a voluntary contraction of the quadriceps. In order to investigate the nature of the quadriceps contraction in effort, I recorded simultaneously the movements of a lever connected with the toecap and the electromyogram obtained by

a string galvanometer connected with needle electrodes plunged into the substance of the quadriceps muscle. During the effort contraction produced by a dynamometer squeeze it will be seen that there is no trace of the Piper vibration, and we are entitled to speak

Fig. 6.



Electromyogram of a voluntary contraction. Below time in 0·01 secs.

of the effort phenomenon as a tonic contraction (Fig. 7). The electromyogram is, moreover, of some considerable interest, as it shows a phenomenon that I have verified in all my records of tonic contractions —at the commencement of the tonic contraction, as evidenced by the movement of the lever, there is a current of action of the order of about 0·1 millivolt. The current of action is of much longer duration than the diphasic variation of a single muscle twitch. Its duration is about 0·4 sec. It will be noted that after it has subsided the galvanometer registers a

Fig. 7.

Upper line is that of movements of foot. Downward movement of lever indicates an upward movement of foot. Lower line is the shadow of galvanometer string. Time in $\frac{1}{5}$ seconds.

perfectly straight line in the position of rest, although the lever registers a continuance of the tonic contraction. It is of some importance to know the nature of the nervous mechanism responsible for this reflex, and this, unfortunately, must at present remain a matter of great uncertainty. Boeke has

found in voluntary muscles nerve endings which appear to be of sympathetic origin, and there is some reason to believe that the centrifugal path of the tonic reflexes is guided by sympathetic fibres; but in spite of the suggestive work of De Boer the question must be regarded as still open. I have found this tonic effort reflex much increased in cases of unilateral pyramidal lesions on the affected side and very much diminished, but still quite definite in cases of tabes. In a case of cerebellar tumour with very marked unilateral hypotonus placed at my disposal by my colleague, Dr. J. S. Collier, the effort reflex was absent on the hypotonic side and normal on the sound side. In the case of the flexed knee, it appears that the tonus of the extensor group is alone increased, while that of the flexor muscles, which by virtue of the position of the leg are already in a state of greater tonus than the extensors, is unaffected or diminished. It is of some interest to determine whether this distribution of tonic innervation occurs elsewhere. If the forearm be flexed at the elbow and supported on a block, and the completely relaxed hand be allowed to hang over the end of the block in a position of supination, it is easy to obtain a record of the hand movements by attaching the lever thread to the head of the middle metacarpal by a pellet of wax. In such a case it will be found that dorsiflexion of the hand occurs in response to effort, whilst when the hand

FIG. 8.

Tracings of A, tonus reflex of flexor; B, of extensor muscles of wrist during mental effort. A downward movement of the lever indicates an upward movement of the hand.

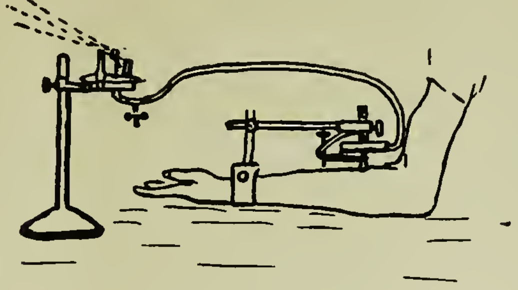
is supinated and allowed to fall backwards over the edge of the block a flexion of the hand at the wrist results (Fig. 8, A and B).

In the case of the elbow-joint, which like the knee-joint only permits movement through one half of a circle, the tonic effort reflex may be demonstrated in the triceps when the upper arm is supported at right angles to the body and the lower arm is allowed to hang downwards. The tonic effort reflex, as shown by the wrist-joint experiments, only affects those muscles which are in a stretched condition. I will next consider what happens when the limb is in a position in which the tonus of the antagonistic groups of muscles is presumably equal. To investigate this point it was obvious that movements of the extremities could no longer serve as indicators; it was therefore necessary to devise some method for the direct measurement of alterations of muscle tonus.

Measurements of Alterations of Muscle Tonus.

It is obvious that for this purpose the registering apparatus must not vary its position in relation to the muscles of the limb, as otherwise a slight involuntary shifting of the limb would be interpreted as an alteration of muscle tonus. To secure this end, in the case of the forearm a suitably devised clamp was fastened firmly to the bony prominences formed by the heads of the radius and ulna. From the clamp a light rod projected backwards to hold a tambour, with a spring attachment that controlled the pressure, with which a short rod was pressed into the flesh over the belly of the muscle to be investigated. The free end of the rod was in contact with the centre of the tambour membrane. Any increase of the tonus of the muscle would press up the rod, and the resultant air displacement in the tambour was conducted by a tube to a delicate Marey tambour fixed on a stand. In place of the ordinary writing lever over this capsule an optical lever was substituted, consisting

Fig. 9.



of a light mirror fastened to the middle of a piece of silk stretched immediately above the tambour. From the back of the mirror projected a short lever 2 mm. in length, actuated by a short reed projecting from the middle of the tambour membrane. By such an arrangement the air displacement, due to movements of the rod resting on the muscle, could be magnified to any desired extent, and a similar apparatus attached to the antagonist muscles allowed simultaneous records of the alterations of tonus to be taken (Fig. 9). The photographic records (Fig. 10) show that with the wrist-joint in the neutral position there is a simultaneous increase of tonus in both antagonistic groups of muscles of the forearm. Similar records have been obtained from the majority of the more accessible skeletal muscles. The effort reflex appears to increase the tonus of all those muscles that are in a stretched or a neutral state. Fig. 11 is a photographic record of the increase of tonus during an attempt to say the "thirteen times" multiplication table. The subject was a very poor arithmetician; the first three or four stages were rattled off with ease and no increase of tonus appeared, but as the difficulty of the multiplication increased the effort showed itself in a gradual

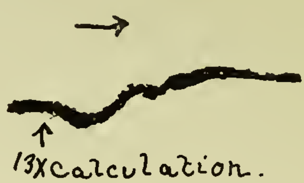
increase of the muscle tonus. I have many records that show a similar correspondence between the degree of tonus and the intensity of the effort put forth. and we shall see later that in certain neurotics who are incapable of prolonged or intense effort the tonus reflex is correspondingly affected. Now as to the significance of the reflex—I have a very strong aversion to all forms of teleological explanations not confirmed by direct observation. One result of the increase of tonus is, however, fairly obvious. The influence of muscular and mental effort in increasing

Fig. 10.

A, increase of tonus in flexors of wrist. B, increase of tonus in extensors. Stimulus between × — × — calculation.

the tendon reflex response is universally admitted, though the current explanation that finds acceptance among neurologists is very unsatisfactory. We are always told that in consequence of the voluntary effort the inhibitory power exercised over the tendon reflex arc by the cortex is relaxed, and hence the response is increased. We know, however, from clinical evidence that the magnitude of the tendon reflex varies directly with the degree of tonus, and I have shown that one sign of effort is an increase of muscular tonus. In order to obtain an exact comparison of any two reflexes it is necessary that the

Fig. 11.

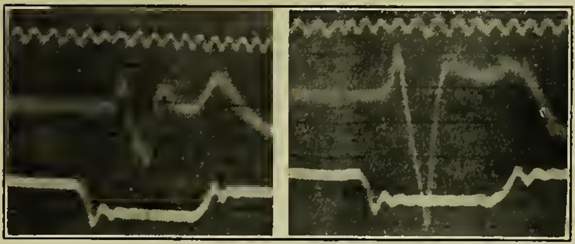


stimuli should be of equal magnitude. I constructed a pendulum hammer which when released at the same angle could be trusted to hit the patellar tendon with uniform force. The moment of occurrence of the stimulus was indicated by the hammer closing an electrical circuit which actuated a Deprez signal when it struck a piece of metal foil that was pasted on to the patella. Fig. 12 shows the tendon reflex at rest and during effort. It will be noted that in addition to an increased response during effort there is a

shortening of the latent period from 20σ to 15σ. This shortening of the latent period presents some difficulty as to its interpretation. If we assume with Piper that the velocity of a nerve impulse is 120 metres per second, and with Jolly and Hoffman that the latent period of the receptor organs and that of the muscle is about 11σ, we obtain values for the synaptic time lost in the cord of about 4σ. Now, the shortening of the latent period in the second of these two reflexes is of such a magnitude that it is very

FIG. 12.

A B



Upper record is the electro-myogram of the knee-jerk, elicited A, when resting; B, when engaged in muscular effort. Lower record is the signal. Time in 1/100th of a second.

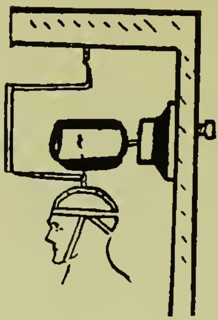
difficult to ascribe it to a diminution of the lost time in the central nervous system, and the same remark applies to the effector muscle and the receptor nerve-endings. Even if we consider that the diminution is spread over all three, its magnitude is very great in comparison to the total time involved. There is no reason to suspect the measurement of the velocity of the nerve impulse of any marked degree of inaccuracy, because it is confirmed by the comparison of the latent periods of the achilles tendon reflex and the knee-jerk in the same individual when the increased length of the latent period in the first reflex corresponds to the additional nerve tract traversed. I have found similar concordant differences in measuring the latent period of the triceps-jerk and the knee-jerk in the baby. Whatever be the explanation, the effort tonus reflex appears to be associated with an increased velocity of response as well as an increase of magnitude, in as far as these simpler reflex mechanisms are concerned. While the diffuse tonic reflex that we have been discussing tends to cause extension of the limbs, the position of the head in effort is differently affected.

EFFORT AND HEAD MOVEMENTS.

To study the head movements the subject was seated in a high-backed chair and to the vertex of a wire skeleton skull cap the free end of a piece of wire was connected by allowing it to lie loosely in a small brass tube soldered on to the vertex. The other end of the wire was fixed in a freely moving ball-and-socket joint on a wooden support fixed directly over the

head. The wire was so curved as to allow a small recording drum to be at right angles to its axis, so that a writing point fixed to the wire immediately above its joint with the skull cap wrote directly on the smoked surface of the drum (Fig. 13). With this arrangement the only head movements that can be recorded are those which take place at right angles to the direction in which the drum is revolving. With this rather primitive instrument I had to obtain two successive records of the response to the same type of stimulus in order to obtain the resultant head movements. I soon found, however, that the lateral movements of the head are of irregular nature and bear no relation to effort. When, however, an effort, whether muscular or intellectual, is made there is a slight but definite forward movement of the head which lasts for the duration of the effort. The absence of the vibratory action currents characteristic of voluntary contraction in the sterno-mastoids has led me to consider this movement to be tonic in nature, but the difficulty of exploring the small flexor muscles of the head will not allow of dogmatic statement. The bowed head observable in certain people when thinking deeply is obviously an exaggeration of this reflex.

Fig. 13.



Effort and Respiratory Movements.

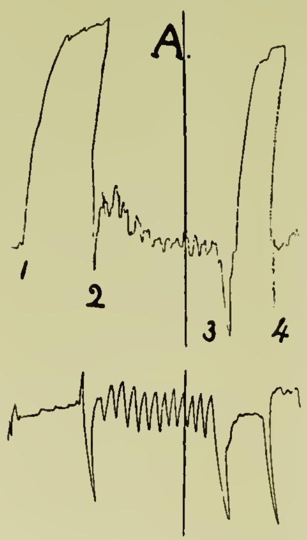
The relation of the respiratory movements to effort has long been known, and a series of observers have studied the alterations of the respiratory curve in the hope of finding distinctions typical of different mental states. Time does not allow me to criticise these records in detail ; I can only subscribe to the generally expressed want of faith in the existence of special types of respiratory curve associated with specific mental states. There is, however, one factor on which everybody is agreed, and that is the reduction of the amplitude of the respiratory movements associated with effort. The usual method of observing respiratory changes by recording the movements of the thorax and abdomen is, however, inconvenient. The apparatus concentrates the attention of the patient on his respiration, and the net effect of the respiratory movements on intrathoracic pressure can only be estimated by the correlation of thoracic and abdominal tracings. In fact, one observer, Rehwoldt, uses no less than five tambours placed on different portions of the thorax and abdomen. The method I have adopted allows the total respiratory variation to be estimated with a minimal amount of disturbance to the subject. A soft rubber No. 1 catheter tube is fixed to the upper lip by a piece of plaster so that its free end is on the same plane as the aperture of the nostril. The tube is connected

with a Marey tambour and the pressure alterations in its air column caused by inspiration and expiration are recorded. After the first few miuutes the subject is almost unaware of the presence of the apparatus, whereas the knowledge that the chest wall is influencing a tambour applied to it produces a curious concentration on the respiratory movcments that as a matter of personal experience I have ncver been able quite to eliminate. The net results of the effect of effort on the respiratory movement are shown in Fig. 5 with a simultaneous record of the quadriceps tonus. The tidal air is reduced to a minimum and the respiration is slowed. We are all aware that when we attempt to perform any very delicate manipulation or some particularly exacting calculation we have a sensation of holding the breath and that the termination of a short effort of this description is followed by a prolonged expiration—a sigh of relief. Some years ago, when I conducted some rescarches on the phagocytosis of carborundum particles uuder different conditions, I found that the respiratory reaction during the performance of counting the particles ingested in a number of leucocytes was the most irksome part of the proceedings. This flattening of the respiratory curve is not to be attributed to a simple diminution of the respiratory movements; it is conditioncd by the general incrcasc of muscular touus, affecting primarily the expiratory muscles, whilst expiration is obstructed by closurc of the glottis. Thc result is an increase in the intrathoracic pressure. On two occasions I have connectcd a manometer with the ncedle in cases in which paracentesis of the thorax was being performed for plcural cffusion. In each case any voluntary effort on the part of the patient was accompanied by a well-marked rise of the intrathoracic pressure. The expiratory and inspiratory oscillations were markedly dccreascd. Turning to the effcct of effort on the intracranial prcssurc we sce what are the results of thesc respiratory changes. In the spring of 1914, together with W. L. Symes, I made some observations on thc alterations of pressure of the cerebro-spinal fluid on a number of cases of cerebral syphilis who wcre being treatcd by the then fashionable remedy of injection of salvarsanised serum. Hitherto all observations of the intracranial pressure have been made with patients who have becn trcphined, and thc pulsating skin and membranes over the trephined area have had their excursions directly rccorded by a tambour. The advantages of the estimation of thc pressure of thc cercbro-spinal fluid by lumbar puncture in thc intact organism over this method are sufficiently obvious, and its dclicacy is very much greater. We examined in all six cases and found that the cerebro-spinal pressurc responds to the slightest physical or intellectual effort by a marked rise. Our observations showed moreover the great influence of rcspiration on the cerebro-spinal pressure. Fig. 14 shows well the enormous fluctuations that occur with deep inspirations and cxpirations and the huge rise whcn the breath is held. The increase of the intrathoracic pressurc during effort is the chief if not the only cause of the concomitant rise of intracranial pressure.

Effort and Circulatory Reaction.

Since the fundamental work of Mosso the response of the circulatory system to cerebral activity has been the subject of a great deal of investigation. The increased frequency of the heart to intellectual effort is well shown in the slide. It is of some interest to discover whether this increased frequency is determined by accelerator stimulation or by removal of the vagal control. For this purpose I took some records with a much faster rate of movement of the photographic paper than in the record shown, and measured the time relations of the systole and diastole. Reid Hunt found that by stimulating the accelerator nerve the times of systole and diastole are decreased together, while with a loss of vagal control the loss of time is confined to the systole. My records all show that the systolic time is unaffected, whereas the diastolic time is diminished; thus the effort is responded to by a diminution of vagal tonus. The blood pressure is again very generally known to be increased in effort, and we shall see that in certain types of neurasthenia this increase is either very small or absent. The criticisms of Pachon have shown that the method commonly practised in this country of taking the records of the pulse at the wrist while the artery is compressed higher up is unsound; this is particularly true when investigating the changes accompanying effort and affective states in which there is reason to believe that vasomotor changes at the extremity of the limb are most marked. On the other hand, to record the pulse at the site of the application of pressure, that is, from the pulsations communicated to the Riva Rocci bag, precludes the use of tambours whose sensitiveness is affected by variations in internal pressure. For this reason I have connected the bag with a mercury manometer and photographed the pulsations (Fig. 15). It will be noted that contrary to the observations of Schrumpf and Zabel the systolic and diastolic pressures are both increased. It does not appear to me to be possible to apportion the relative degrees of influence exerted by the heart and the vasomotor system in this reaction.

Fig. 14.



Simultaneous records of the cerebro-spinal pressure (upper line) and the respiration (lower line). Between 1 and 2 breath was held, 3 and 4 are deep inspirations.

The vasomotor changes in the peripheral circulation in response to cerebral activity have been much studied, and the observations of Lehman and Weber—to mention only two of the writers who have most concerned themselves with plethysmographic studies

FIG. 15.

Photograph of pulse oscillations of mercury column at different pressures of Riva-Rocci bag A when subject is at rest, B during calculation. The arrows point to beginning of pulsations and to the maximum oscillation.

—agree in recording a fall in the volume of the limb as a sign of effort. The vaso-constriction continues throughout the period of effort, and should the effort have been sufficiently intense to cause discomfort

its cessation is followed by an increase in the limb volume to a greater size than before the beginning of the effort. We shall consider this after-effect when we study the objective signs of affective states. Weber has recorded alterations in the intra-abdominal pressure accompanying cerebral activity which he ascribes to a variation in the blood-supply to the intra-abdominal viscera.

Effort and Visceral Reaction.

What we have seen of the increased muscular tonus during effort and the respiratory variations makes any interpretation of results obtained by introducing balloons into the hollow viscera exceedingly difficult. That many important visceral changes accompany cerebral activity is, of course, certain, in fact, they constitute the most important part of the bodily accompaniments of such activity, but so far their investigation has to be conducted by methods adapted to show chemical rather than gross motor variations. Lange has described fluctuations of the " active sense attention "—sinnliche aufmerksamkeit—the existence of which is shown by the fluctuating appearance and non-appearance in consciousness of a minimal sense impression. I have objectively demonstrated fluctuations of effort; for this purpose the subject was instructed to hold a wand in his hand and to keep the point of it in the middle of a small ring, suspended from a stand. The ring was lined on the inside with a hollow thin-walled rubber bag, which was blown up to a low degree of tension and connected at its orifice with a recording tambour. The quadriceps tonus and the respirations of the subject were recorded on a drum together with the pressure in the bag. A slight movement on the part of the subject would lead to the wand touching the bag and the increment of pressure was recorded by a movement of the tambour lever. When the touches on the ring bag were most frequent there was a diminution of the quadriceps tonus and an increase in the depth of the respirations. The back of the subject was turned to the drum so that he could not observe what was taking place. In this fashion it was possible to demonstrate fluctuations of the effort coinciding with periods of inaccuracy in the handling of the wand. Such periods became more frequent as the subject became fatigued, and in certain cases of neurosis complaining of nervous asthenia they were particularly frequent.

I have so far endeavoured to give some brief account of the movements that are the objective expression of effort. The first portion of my next lecture will be devoted to the consideration of the relationship of such movements to the sensory phenomena that are the concomitants of mental and physical effort.

LECTURE II.

In the preceding lecture we reviewed some of the objective aspects of effort. Before proceeding to study the other bodily concomitants of cerebral activity, I wish to summarise what objective study has taught us of the state of activity that we designate as effort. In the first place, all the phenomena recorded were in the nature of increased bodily activity and not of the inhibition of activity, that it might be thought would permit the more readily the uninterrupted response of the specific mechanism to the stimulus. We found that the generalised bodily reaction of effort bore a direct relation to the difficulty —that is, to the chance of failure of the specific mechanism in performing its appropriate action and we noted that when this bodily response experienced any diminution, the accuracy of the specific performance was likewise diminished. Lastly, we found that the net result of such of the bodily changes observed in effort was to increase the general efficiency of the body, whether by accelerating and increasing the simpler motor reflexes or by influencing the blood-supply of the nervous system by an increase of the intrathoracic pressure and vasomotor changes.

Now let us interrogate our consciousness as to the feeling that we experience during effort. As in the case of other affective states that we shall study later, we find that to attend to a feeling is, to a great extent, to lose it. We are aware of certain respiratory sensations, a certain sense of increased cardiac action, of intracranial pressure, and of postural tension. Sometimes one, sometimes another of the constituents of this feeling of effort becomes so dominant that the vague feeling is swamped by a distinct sensation such as I instanced in the case of the respiratory symptoms experienced in performing a count of the particles ingested by phagocytes. The general sum-total of the sensory presentation of the bodily changes of effort is a disagreeable one. The sooner that we can cease from effort and find our specific task performed the better pleased we are.

Ribot has emphasised the fact that nobody really has pleasure in work itself, it is to its accomplishment that we really look forward with pleasure. Now whether we regard effort from a purely mechanical point of view, or whether we take cognisance of its affective aspect, the conclusions that will be drawn are much the same. A stimulus is presented, the specific response to which is inadequate, or else the task would be accomplished with mathematical certainty. When such a stimulus fails to arouse an adequate response on account of its own feeble nature, nothing further happens except that the response will itself be a feeble one. When, however, the stimulus is powerful and insistent and the mechanism of response is inadequate another and more diffuse constellation of reflexes is aroused. Various bodily activities are brought into action, all of which tend

to increase the efficiency of the specific mechanism. The sensory presentation of these bodily activities is a disagreeable feeling which we do our best to allay by accomplishing the specific action demanded. Viewed from this angle we shall see that the mechanism of effort resembles that of the affective states that we are about to consider, and further, that no distinction can be drawn between effort and the conception of attention as propounded by Locke and in a less incisive fashion by Hamilton. It will be obvious that when through either innate or acquired disability the specific mechanisms are everywhere inadequate to respond to the tasks that they are called upon to perform, the never-ceasing activity of the mechanism of effort will prove exhausting to the body, and in its sensory aspects a most distressing form of neurotio disturbance.

Reaction Manifested by Change in Electrical Properties of the Skin.

Hitherto the reactions that we have studied have been occasioned by the activity of striated or non-striated muscular tissue. I now propose to deal with a reaction that is manifested by change in the electrical properties of the skin. This reaction was first described by Féré in 1888 as an emotional response, manifesting itself by a diminution of the resistance of the body to the passage of an electric current. Galvanometric deflections occurring when the skin was explored by a pair of electrodes were independently described by Tarchanow in 1890, and found by him to occur as responses to physical and psychical stimulation. He considered them to be due to secretory currents of the skin glands.

The investigations of both of these observers were to a great extent ignored, and the same fate befell Stricker's confirmation of Tarchanow's experiments in 1902. It was not till 1904 that the subject was reinvestigated, when Veraguth, stimulated by some preliminary observations on bodily resistance by E. K. Müller, confirmed the observations of Féré and gave the first systematic exposition of what he termed the "physico-psycho-galvanic reflex." He described this reflex as a diminution of the resistance of the skin to the passage of a constant current occurring as a response to stimuli which caused an alteration of the affective state. It is not possible in the course of these lectures to deal with the mass of observations that have since accumulated from the work of the many experimenters who, subsequently to Féré and Veraguth, have investigated this phenomenon. Nor are we concerned for the moment with the physical nature of the response. It is sufficient to state that the galvanometric effect is beyond all doubt due to a diminution of the electrical resistance offered to the passage of a constant current by the skin, and that this diminution is exhibited as a maximum effect in the palms of the hands, a little less obviously in the sole of the foot, and scarcely at all in other parts of the skin. This statement, which is founded on the careful exploration of all parts of the body surface

by Gildermeister, has been confirmed by every observer who has worked on the subject.

The work of Crile and his pupils has shown that an increase of electrical conductivity, or conversely a diminution of resistance, is associated in all tissues, with an increase of their metabolic activity; we may tentatively regard the galvanic response as a sign of the increased activity of the gland-bearing area of the skin. If it be to the activity of the cutaneous glands themselves that we must ascribe the galvanic reflex it is certainly not due to the gross manifestations of that activity by the excretion of sweat. In 1918 I had a neurotic patient who suffered from hyperidrosis of both hands, the sweat falling off from the tips of the fingers in drops. I found that this patient exhibited the galvanic response to all adequate stimuli in a marked degree, although the palms were covered by a thin layer of sweat, and it was unthinkable that any actual increase in the amount of fluid excreted should have an influence on the conductivity of the sodden skin. I showed the case to Prof. Waller, and he also remarked on the magnitude of the galvanic response given by this patient. It is further obvious that the galvanic response, if it be associated with glandular activity, can only be governed by the centrifugal nerve impulses that travel through the fibres of the sympathetic, and we shall later see reason to believe that, in common with all the other mechanisms which take part in the general bodily reaction to nocuous stimuli, the galvanic reaction is conditioned by the response of the thalamic system. That changes in the peripheral blood-supply have nothing to do with the response was shown by the experiment of Veraguth, who obtained normal responses from a limb exsanguinated by an Esmarch bandage.

I now propose to deal with some of the aspects of the galvanic reflex that will prove of service in the objective study of the bodily response to cerebral activity. Before doing so, I will indicate briefly the technique that can be usefully employed for this purpose. In my own experiments I have been in the habit of measuring the bodily resistance to a current of a couple of Leclanché cells which is passed through the body from two liquid electrodes of normal saline into which zinc terminals dip. The subject is seated with the hands immersed one in either electrode, and forms the fourth arm of the familiar Wheatstone bridge circuit. I prefer the liquid electrodes to the solid zinc electrodes strapped on to the back and front of a single hand, because slight shifting of the electrodes due to movements occurs rather easily when the latter procedure is adopted; further, the low resistance measured allows its easy demonstration to the uninitiated without having to multiply up the results by using unequal ratio arms in the resistance box. The point is, of course, of no importance. For experimental work I have abandoned the moving coil galvanometer for the Einthoven instrument, since with this type of galvanometer it is alone possible to measure time relations with any degree of accuracy, and an additional interest is given to the records by

the appearance of the heart beats in the curves. Further, with the double-string instrument which I owe to the generosity of Mr. Alfred Teniers it is possible to obtain either simultaneous records of the galvanic response of two parts of the body or of muscular or respiratory variations accompanying the galvanic reflex.

Nature of Stimulus Evoking Galvanic Response.

The first subject for inquiry is as to the nature of the stimulus that evokes the galvanic response. Practically all those who have concerned themselves with this reflex agree that stimuli adequate for the galvanic reflex are capable of causing an affective state. We are in no way entitled to draw the inference that the occurrence of an affective state is necessary to the exhibition of the reflex, and still less that the occurrence of the reflex conditions an affective state. While the examination of the relation between bodily activity and states of feeling may be reserved at this stage for later discussion, it must here be pointed out that an affective state, to exist at all, must be experienced by the subject. Now, a galvanic response may occur without the subjective experience of an affective state. I have frequently observed a response to be elicited by a stimulus when the subject affirms that the stimulus has for him no feeling tone. This may be best observed in the performance of word association experiments. The response to a certain word may be marked and yet when the subject is interrogated he is at first unable to discover any association of emotional significance in the word, and, moreover, he assures me that the word left him absolutely indifferent. It is true that further examination generally succeeds in recalling a hidden emotional significance, and thus reveals that the stimulus activated a specific reflex system that at one time had a nexus with the mechanism of feeling ; but that is not the point. If we accept the assurance of the subject, we must believe that the reflex occurred without his experiencing any feeling connected with the word. To talk of a subconscious feeling is, of course, to talk nonsense. The same point may be verified by purely introspective investigation. When a painful or presumably painful stimulus is applied to the hand, such as touching it for a moment with a flaming match, a well-marked reaction will be obtained.

If, however, the experiment be repeated a number of times in succession without the injury inflicted corresponding to that which was anticipated at the first stimulation, the subject becomes indifferent and possibly bored with the performance, and yet with each application of the stimulus there is still a galvanic reaction though the subject is well assured that he feels no apprehension. To appreciate the cogency of the experiment one must oneself serve as a subject, and there is no mistaking the genuine surprise with which one observes a reaction to a stimulus that has become a matter of indifference. I shall refer to this experiment again when we come to discuss conditioned reflexes. Here its importance lies in the

fact that the reaction takes place in the absence of an affective state. Many observers have maintained that the galvanic reflex accompanies mental effort as well as states of feeling. We have just now discussed the sensory concomitants of effort and I hope to have succeeded in demonstrating that they constitute what we know as an affective state ; but I do not consider that they are normally accompanied by a galvanic reaction. When, however, an intense effort results in one or other of its sensory factors assuming overwhelming proportions, we have said that it is presented as a distinct disagreeable sensation and may as such arouse the same system of reflexes as it would were it a sensation caused by a specific stimulus. In this connexion I instanced the feeling of suffocation during a cell count. In the performance of word association tests I have generally found that the first three or four word reactions in the examination of a new and nervous subject are accompanied by a galvanometric deflection, even though they be essentially neutral words. This is simply an expression of the state of anxiety on the part of the subject, who soon settles down and responds electrically only to words having an emotional significance.

Partial Clearing up of Confusion Between Subjective and Objective Standards.

These considerations indicate some of the difficulties to be encountered in any attempt to elucidate the meaning of a physical response by an appeal to subjective evidence; a similar confusion of subjective with objective standards has proved to be such a stumbling-block in the physiology of the afferent nervous system. In that case the problem for the physiologist is not what is the presentation to consciousness following on the stimulation of an afferent nerve-ending, but what are the reflexes that such stimulation evokes, and by what form of stimulus is the receptor organ excited. Thanks to the introduction of the idea of the existence of a system for the transmission of nocuous stimuli by Sherrington, and its correlation with the thalamic reflex system by Head and Holmes, the physiology of the nociceptoral nervous system has been made relatively clear, and we have eliminated many of the obscurities that were so perplexing when the subjective sensations of the patient were our only guide. Likewise, in considering the galvanic reflex the first question to be considered is what are the objective characteristics of the stimuli that evoke this increase of conductivity on the part of the skin—an increase which we have reason to consider to be an index of a general increase of functional activity and to be correlated to certain states of feeling. The answer is that in the first place any type of stimulus that tends to cause damage to the body, or that threatens such a change in the environment as is likely to result in bodily damage, will prove to be an adequate stimulus. In the second place we must recognise not only actual bodily damage but the disturbance of the smooth working of bodily activities as the characteristic of another class of stimuli which will evoke the generalised

bodily reaction of which the galvanic reflex is a sign. The first class of these stimuli is represented by such physical disturbances as may result from a blow, a burn, a prick or pinch ; or those acting from a distance, such as loud sounds, blinding lights, or noxious smells ; or, again, endogenous stimuli, such as colic or neuralgia.

Fig. 1 shows a typical reaction as registered by the string galvanometer to a prick, the initial muscular

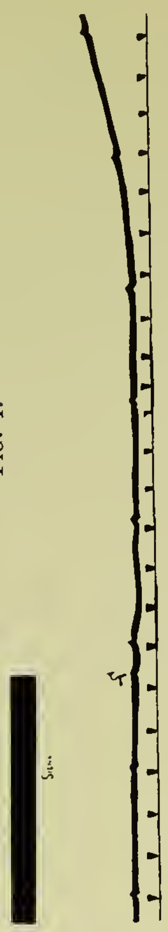


Fig. 1.

Galvanic response to a prick. Observe start (S) at the moment of stimulation. The heart beats are clearly shown. Time interval one-fifth of a second.

start of which we shall have more to say is clearly seen. If now we proceed to use verbal stimuli instead of actual physical procedures we are using a type of stimulus belonging to the second category of our classification. The simplest form of verbal stimulus will be the threat of the application of a nocuous physical stimulus. Fig. 2 shows the responses to the threat of a prick and to an actual prick.· Here

we might hope to obtain some light on the relations of the two nervous mechanisms aroused. In this particular case you will note that the response is as great to the threat as it is to the act. I should like to be able to say that it was possible to correlate the mentality of my subjects with the relative efficiency of the two stimuli, but the results obtained from a large number of observations have been too inconstant. There are probably too many factors involved in the experiment—the degree of dramatic conviction with which the threat is performed, the attention of the patient, and the relative severity of the actual physical stimulus, must all be variables.

The threat to cause bodily discomfort is the simplest form of verbal stimulus. From this point onwards the stimuli may be elaborated by the representation of circumstances either past or present that are opposed to the well-being of the subject or are in conflict with the general tendency of his conduct—that is, stimuli such as will on the affective side evoke the feelings of grief or anger. So much for the galvanic response to external stimuli, but even more important is the galvanic response to stimuli whose immediate origin is in the autogenous neural processes of the subject. The spontaneous representation of some disagreeable or painful circumstance will serve equally well to elicit the galvanic reflex. Whether the subject recalls silently some past experience of a painful nature or whether he communicates it verbally to the observer the representation will in both cases evoke a diminution of skin resistance. We can now determine objectively the nature of the stimuli that will evoke the reflex without any further appeal to the evidence furnished by subjective inquiry. We note that one and all the stimuli have this property in common—their continuance is inimical to the well-being of the subject. Obvious as this is for the physical stimuli, it is no less clear when we consider the stimuli derived from outside verbal suggestion or from the representations evolved by the subject's own activities. The representation of an old pain must, if it be sufficiently vivid, involve all those reactions that occurred with the original stimulus. In so far as it falls short of representing them it falls short of being a vivid impression. I need hardly remind you

FIG. 2.

Galvanic response to burn and threat of burn. Graduations of diminution of resistance by 200 ohms.

that a representation must call into activity those nervous mechanisms that were aroused in the first instance, and that their necessary modification by subsequent neural associations constitutes the difference between the actual and the revived experience. In so far as the bodily response is concerned, the stimulus will in both cases be of the same nature as the initial physical stimulus, and from the objective point of view the reflex increase of conductivity evidences the same type of nervous activity. Fortunately for ourselves the representation can never or very seldom occur in anything approaching its first intensity. In many instances it cannot recur at all in a normally constituted organism.

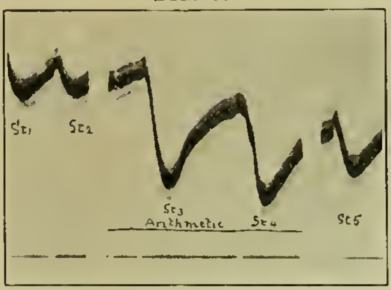
The Galvanic Reflex not under Voluntary Control.

The completeness of representation is the test of the degree in which the activities of the organism as a whole have been modified by the original experience. I try to recall an old toothache and I succeed in recalling concomitant experiences, but not a twinge of the original toothache. I recall a profound grief, and I shall feel much of my pristine pain and sorrow. Now the galvanic reflex distinguishes between these two types of representation, and this is what makes it so valuable for the objective study of cerebral activity; in our inner lives we are not always quite sure ourselves as to whether we have really any feeling about a subject—that is, whether it evokes a bodily resonance or whether it is merely a subject about which we think that we ought to feel strongly, whereas, in fact, it fails to arouse any bodily response. It is the objective demonstration of these two states that is furnished by the galvanometer. Anyone who cares to sit quietly by himself and recall past experiences whilst connected with the galvanometer circuit will learn much about matters that he has hitherto taken for granted. The resistance reaction is not under voluntary control; it is impossible to evoke it by simulation of affective states, such as a fictitious rage, nor can the mere recitation of emotional poetry, no matter with what emphasis it be declaimed, produce a reaction unless by some chance a phrase acts as a stimulus to evoke some association with a personal experience of affective import.

I will now pass on to consider some of the other aspects of this reflex that are of importance in affording information as to the relations of the mechanism of bodily reaction accompanying affective states to other nervous mechanisms. The reflex cannot be inhibited by any voluntary effort on the part of the subject. I have sought for evidence of inhibition either of the response to physical or verbal stimuli in over a hundred subjects, but have never met with evidence of any direct voluntary power to either inhibit or modify the reflex. When the stimulus is not a maximal one the response may be modified to some extent by the activities of the subject at the time of stimulation. Fig. 3 shows the response to a moderately loud auditory stimulus, first, when the subject was expecting the stimulus; next, when he

was engaged in mental arithmetic ; and lastly (Fig. 4), when he was subjecting himself to a very painful faradisation applied to the sole of the foot. When I first obtained this result it seemed strange to me that the response to an irrevelant stimulus should be greater during the performance of arduous work than

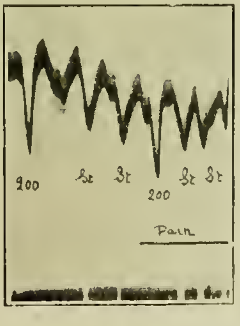
FIG. 3.



Reaction to uniform stimuli (prick). 1 and 2 before, 3 and 4 during, and 5 after mental arithmetic.

when the nervous system was keyed up to expect the stimulus. The work performed, be it noted, was not responsible for any diminution of resistance per se. I have, however, constantly obtained this modification of the reaction to moderate stimuli, slight though it be, so long as the work on which the subject is engaged does not prove unduly irksome. When the resentment or the discomfort of strenuous work

FIG. 4.

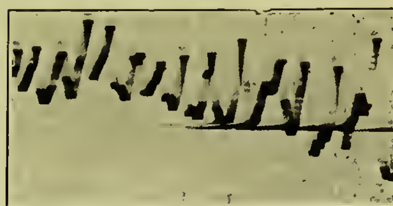


Reaction to uniform stimuli (prick) before and during painful faradisation. Graduation of 200 ohms.

reaches such a pitch as in itself to produce a bodily reaction in the shape of a diminution of resistance, the response to the initial stimulus is diminished, just as we see occurred in this record when the subject was undergoing painful stimulation. When, however, the stimulus is a maximal one, such as is produced

by the firing of a pistol close behind the subject, it ceases to be possible to modify the response either by work or by any moderate degree of pain. Fig. 5 is tbe record of wbat is apparently a fatigue effect in tbe galvanic response to a series of uniform auditory stimuli. Tbis apparent fatigue cannot be attributed to exhaustion of tbe effector mechanism of tbe response, that is either to the skin, the centrifugal nerves going thereto, or tbe nerve cells governing them, for if an adequate stimulus of a different nature to that of tbe series be administered at tbe beginning and at tbe end of the series it will be found to evoke approximately the same degree of response. Under the conditions of the experiment, which consists in

Fig. 5.



Fatigue effect of galvanic reflex to uniform series of sound stimuli.

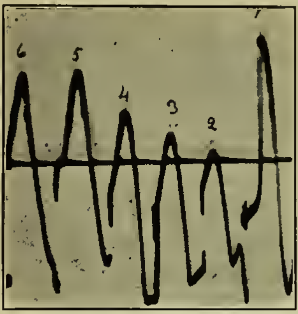
tbe production of a short series of sounds of equal and moderate intensity, tbe effect cannot be due to fatigue eitber of the peripheral receptor mechanism or of tbe cortical receptor centres. We may therefore ascribe tbe progressive diminution of tbe response to some change occurring in the nervous mecbanism connecting tbe receptor system witb tbe effector system of tbe galvanic reflex. The term "progressive indifference" probably describes this change better tban fatigue; for if in the middle of the series of moderate stimuli we intercalate one of greater magnitude we obtain a response approximately equal to that aroused by a stimulus of the same magnitude administered before tbe beginning of the series. It is notewortby that after the intercalation of the powerful stimulus tbe descending series of responses is interrupted for tbe next two stimuli wbicb show some degree of augmentation.

Variation of Response with Stimulus.

Wben tbe patient is subjected to a brief examination not lasting over ten minutes or so the magnitude of tbe response can be shown to vary directly with tbe strength of tbe stimulus. Fig. 6 sbows the response to a series of break induction shots of increasing strengtb. This relation between the strength of the stimulus and the response is obviously a fact of some importance, but we must hesitate before we pronounce on the relative efficiency of any two verbal stimuli or stimuli evoked by the subject's own representations on tbe strength of an examination of the galvanic responses elicited. Even with simple and easily controlled physical stimuli two such stimulations of

different strengths can only be counted upon to give proportionate responses when they are not separated at too great an interval and the general attitude and condition of the subject is approximately the same. After an initial intense stimulus with its concomitant response the resistance does not immediately rise to the old level, and so long as the slight general diminution in resistance continues the response to subsequent stimuli, of whatever their nature, will be somewhat greater than formerly. In the study of the galvanic reactions of neurotics undergoing a prolonged examination this tendency must not be overlooked.

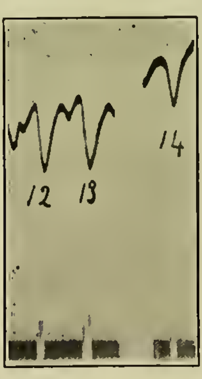
FIG. 6.



Response to series of break induction shocks. 2 coil at 10 ; 3 coil at 8 ; 4 coil at 6 ; 5 coil at 4 ; 6 coil at 2 ; 1 graduation 400 ohms. Final graduation omitted.

If a painful stimulus be repeatedly administered, and its administration be accompanied by some sound or movement on the part of the observer, after a time the repetition of this sound or movement will be in itself sufficient to produce a response equal to that caused by the stimulus. We shall, in fact, have established what Bechterew calls a conditioned reflex. Fig. 7 shows such a conditioned reflex. A painful stimulus was administered to a subject who was instructed to press a key which completed the circuit of an electric signal every time that he felt the stimulus. In addition to the motor reaction the galvanic reaction to the stimulus was recorded. Every time that the painful stimulus, which was an induction shock, was administered, the subject who sat with his eyes closed heard the click of the key that opened the

FIG. 7.



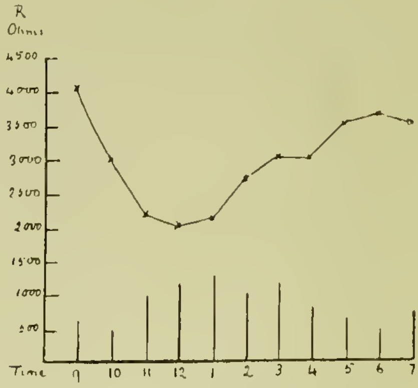
A conditioned reflex.—Upper line, the galvanometer, lower broad line the signal response. 12 and 13 are the last in a series of painful stimuli. For 14 the stimulus is short-circuited, but patient still responds on hearing the click of the key.

circuit for the break induction shock. After the reaction had been repeated a sufficient number of times the key was clicked as usual, but the current was prevented from reaching the subject by a previous disconnexion of the circuit. On hearing the accustomed click the subject reacted in the accustomed fashion both by the signal and by a galvanic response, which will be seen to have been of no less magnitude than that responding to the last two occasions of the series when the induction shock was really administered. We shall see in the next lecture what light a consideration of these conditioned reflexes throws on the relation between the bodily response and the affective state.

Problem of Skin Resistance.

The relation of the initial resistance of the skin to the magnitude of the galvanic response is a very difficult point to determine. Waller has shown that the skin resistance undergoes a diurnal variation following closely the diurnal variation of temperature. I took advantage of this observation to determine the relation of the galvanic responses evoked by

Fig. 8.



Graph showing rough correspondence through the day between skin resistance and galvanic reflex.

uniform stimuli to the initial skin resistance. Maximal stimuli were used, and for this purpose I found a pistol shot most convenient. A rough correspondence between the resistance of the skin and the galvanic reflex throughout the day is shown by Fig. 8. This chart represents the mean of observations conducted on three successive days, but the difficulties in obtaining the subject at any one time under approximately the same conditions are very great. For what it is worth, it seems to indicate clearly that the maximum response occurs round about mid-day when the

resistance of the skin is at its lowest. How far we are justified in assuming that cerebral activity is at its maximum at this time is very doubtful. Most people have the impression that their work is better performed either in the early morning or late at night, but this opinion is perhaps of little value. We judge of our work after all by much the same standards as those by which the world at large assesses our personal value. Everyone knows that the chief conditions of worldly success are great industry combined with a fair amount of stupidity, or—to put it less bluntly—with an absence of that self-criticism that so often destroys the power to accomplish things by leading us to inquire as to whether they are really worth doing. Similarly, the greater facility with which work is conducted at hours remote from the mid-day interval may simply be an expression of the abeyance of similar critical and inhibitory processes.

I have been unable, in spite of the large amount of material investigated, to secure any data as to a correlation between intelligence and the galvanic response that would appear to be sufficiently trustworthy to justify its inclusion in this review. In this connexion the work of Miss Waller appears to be highly important. She examined the galvanic responses of a class of 70 students to a series of standardised stimuli. The students shortly afterwards underwent an examination in physics. Correlating the marks obtained and the magnitude of the galvanic responses Miss Waller found that the average response of the higher placed half of the class in the results of the examination was greater than that of the lower half of the class. A further investigation on these lines is at present urgently needed.

Importance of Latent Reaction Time.

The latent time that occurs between the stimulus and the galvanic response is, as we shall see later, a matter of great importance in the elucidation of the significance of the bodily reactions. I have a large number of records, taken with the Einthoven galvanometer, which show the latent period in response to a variety of stimuli. To a physical stimulus, such as sound, the reaction time may be as long as 2·5 seconds, when the stimulus is a single loud tap on the table, whereas it may be as short as 0·8 second to a pistol shot; the latter figure was given by a young man with very large galvanic responses who was suffering from diabetes, a condition which we are accustomed to associate with hyper-excitability of the sympathetic system. The duration of the response is, in the case of a physical stimulus, conditioned primarily by its initial magnitude. As we have already noted, a response of great magnitude may be followed by an after-effect lasting for a considerable period, often three or four minutes. When we are dealing with verbal stimuli the latent period may be very long, the association processes necessary to evoke the response may be complex, and the duration of the response may be greatly lengthened owing to perseveration.

Specificity of the Galvanic Reflex to Nocuous Stimuli.

I have now dealt with some of the main facts that have so far presented themselves from the study of the galvanic reflex, and I have especially dwelt on the nature of the stimuli that evoke this reflex. We have seen that to divide them into physical and psychical stimuli is from the objective point of view unjustifiable, since all these forms of stimulus have in common the property of injuring or inhibiting the normal course of vital activity. In future, in speaking of such stimuli, it will be convenient to assign to them collectively the same name that Sherrington has bestowed on those belonging to the physical class, and to refer to them as nocuous stimuli. The galvanic response is not the only type of response to nocuous stimuli that we know of. We have seen that it is an inevitable response incapable of any great degree of modification by other forms of activity, but this characteristic it shares with a number of other responses that we may study objectively. It has, however, one characteristic that at once makes its study of paramount importance in any objective investigation of neurosis, and that is that, alone of all the forms of bodily response that we shall study, it is specific to nocuous stimuli. It does not primarily occur as a bodily sign of the state of cerebral activity that we have considered already as effort.

Other Objective Signs of Response to Nocuous Stimuli.

Now, in investigating further objective signs of the response to nocuous stimuli we shall find that we have met with all these signs in the study of effort. The diffuse increase of muscular tonus that we have already seen to be manifested both by movements of the extremities and by induration of the muscle occurs also as a response to nocuous stimuli of sufficient intensity. Fig. 9 shows a record of the movement of the foot caused by increased tonus of the

Fig. 9.

quadriceps as a response to a painful stimulus. Fig. 10 is the photographic record of the increase in tonus of the arm muscles to a nocuous physical stimulus recorded by the optical lever. The movements of the head recorded by the method described in the previous lecture are, however, of a nature different from that of those elicited by effort. You may remember that the tonic response to effort was manifested by a forward movement of the head.

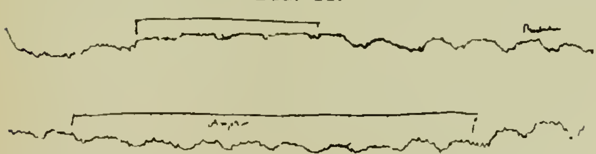
A painful stimulus is responded to by a retraction of the head. This might appear to be an example of a purposive movement, the subject endeavouring to withdraw from the vicinity of the painful stimulus. But on applying the stimulus to the back instead of in front the same retraction of the head occurs, though in this case the subject is moving in the direction of the stimulus. It cannot, therefore, be

FIG. 10.

Photographic record of the increase in tonus of the arm muscles to a nocuous physical stimulus, recorded by the optical lever.

compared to the type of reflex that occurs when a spinal animal withdraws the foot from a nocuous stimulus. Disagreeable and offensive verbal stimuli will also elicit this retraction reflex (Fig. 11), which—in contradistinction to the forward movement accompanying effort—appears, therefore, to have some of the specificity to nocuous stimuli that characterises the galvanic reflex. The respiratory

FIG. 11.



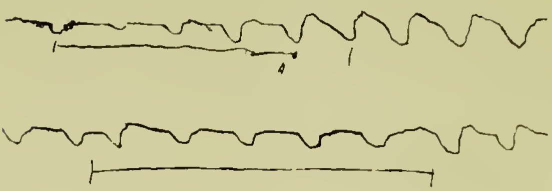
Opposite movements of head. Forward with effort (recitation). Backward in response to offensive (verbal) stimulus. Respiratory and circulatory movements shown.

changes do not differ, so far as I have been able to observe them, from those associated with effort. Fig. 12 is the record of two interesting tracings obtained, one as the reaction to a painful thought and the other to anger. The response in both cases is manifested as a flattening of the respiratory curve that we have seen reason to associate with increased muscular tonus. I am aware that many observers have attempted to demonstrate the existence of special forms of the respiratory curve associated with specific affective states, but the lack of uniformity in their results, which depend on measurement of the expiratory and inspiratory excursions, have failed to convince me that they are dealing with any specific reflex changes. The heart rhythm is accelerated in

response to nocuous stimuli, but such acceleration, when it occurs, is of very short duration in response to a short-lived stimulus. With prolonged stimulation the cardiac rhythm is accelerated for the whole duration of the stimulus. Again, if the inferences drawn from measurement of the time relations of the cardiac cycle are to be trusted, it appears that the acceleration is due to depression of the vagal inhibitory and not to stimulation of the accelerator mechanism.

The evidence furnished by plethysmographic tracings of the vaso-motor reactions occurring in the limbs must be accepted with a certain amount of reserve. In the well-known Mosso plethysmograph as modified by Lehmann there are two possible sources of error which it is practically impossible to eliminate. First, a very small amount of displacement of the arm will cause a very marked effect on the plethysmographic curve and will be interpreted as an increase of volume if the arm be moved forward and as a decrease of volume if the arm be withdrawn

FIG. 12.

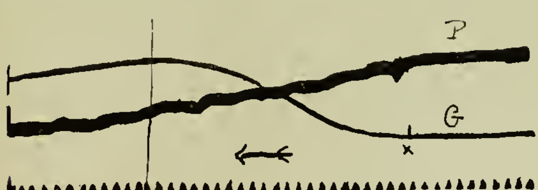


Respiratory tracings. The upper tracing shows effect of anger. The lower that of grief. The period during which the subject was indulging in the thought of the vexations and the painful experiences is indicated by the straight line below the tracing. (To be read from right to left.)

ever so slightly from the plethysmograph. That such slight movements do occur and cannot be consciously guarded against is shown by the old experiments with an apparatus known as the "automatograph." In these experiments the partially flexed forearm is suspended in a cradle hung from a support. The movements of the cradle are recorded on an underlying drum and it is claimed that by this method it is possible to show that a disagreeable stimulus is responded to by retraction of the arm and an agreeable stimulus by its advancement. I have not adopted this method in my investigations, because I consider that it harbours many fallacies; but whatever the interpretation we may place on them, results thus obtained show that such movements certainly do occur. Another source of fallacy in the use of the plethysmograph becomes obvious from the evidence that I have submitted as to the diffuse increase in muscular tonus occurring as a bodily reaction in certain states of cerebral activity. Such an increase in muscular tonus must occasion an alteration of muscle volume simulating a vaso-constriction. I have dwelt on the shortcomings of the plethysmograph because we shall see later that results obtained

from this instrument are the chief support for the assumption of a bodily response occurring in exactly the opposite direction to that elicited by nocuous stimuli. In order to avoid one source of error—that arising from slight movements of withdrawal or dropulsion of the limb—I have designed a form of plethysmograph that has yielded fairly satisfactory results in these experiments. It is in essence a light metal drum covered at either end by a stout rubber diaphragm. There is a circular hole in each diaphragm, large enough to allow the passage of the arm whilst permitting a firm gripping of the limb by the thick rubber. The arm is thrust through both diaphragms so that the hand and wrist project beyond the further one, whilst the proximal end is a couple of inches below the elbow-joint. The drum is connected to a type of Brodie bellows recorder in which the bellows is replaced by a thin rubber "condom." The plethysmograph hangs freely on the arm, which is supported by two blocks at the wrist and elbow. Using an instrument of this type, which cannot be affected by any slight movements of the arm, in the direction of withdrawal or propulsion, I have obtained records of diminution of limb volume that are in accordance with those obtained by previous observers. It will be noted, however, that this arrangement of the plethysmograph cannot altogether neutralise, though it sensibly reduced, the error that arises from changes in muscle volume as a result of increase of tonus. The only method of plethysmographic recording that would be free from both these errors is that adopted by Weber who used an ear plethysmograph. I confess that the extremely disagreeable features of this method, which involves the working of large amounts of vaseline into the

Fig. 13.



Simultaneous record of galvanic (G) and plethysmographic (P) responses to a prick, x.

hair, have prevented me from proposing its use to the subjects of my experiments. Fig. 13 shows a photographic record of the plethysmographic and the galvanometric responses to a nocuous stimulus. It will be observed that the latent period is identical in the two forms of response.

Responses to Benign Stimuli.

Now so far we have considered the bodily response to nocuous stimuli, that as to stimuli that tend to disturb and destroy life, and we have, moreover, noted that the sensory concomitant of such responses

has been a feeling of an unpleasant nature. We may now fairly envisage the question as to what are the responses to stimuli that tend to further and perfect life—what we will call benign stimuli—which so far as they have any sensory reaction affect us pleasantly. Since the galvanic reflex is the only response that we have so far been able to study that is specific to nocuous stimuli and is not elicited by effort, it is to this reflex that I shall first turn for an answer. I have found that to stimuli of a benign nature there is either no response at all or else a slow increase in resistance occurs; but this slow alteration of resistance is only to be noted when the preceding state is one of the low resistance that we consider to be indicative of general bodily activity. If, after a series of painful or vexatious stimuli the attention of the subject be diverted to more pleasurable matters, the lowered resistance rises gradually until a point above the initial neutral state has been reached. If a hungry subject be given a satisfying drink and appetising food the resistance will show a slow increase. If a subject who has taken up his position, rather dreading a series of irksome experiments with painful electrical shocks, be told that for that séance the observer only purposes to take some simple pulse records this welcome news is followed by a slowly developing increase of resistance. The response with which we are dealing has none of the brusque and decided characteristics of the galvanic response to nocuous stimuli; its slow and variable onset and the indefinite nature of the stimuli that tend to arouse it do not allow of any formulation of the latent period of this increase of resistance. I have never seen it occur under five seconds with the type of stimulus that I have been considering. This indefiniteness may partly be attributed to the method of experiment—to attempt to produce such a violent reaction as that which we characterise as joy is, under the conditions of the somewhat dreary experiments conducted in the laboratory, a practical impossibility. The muscular tonus response that has been found to be evoked by both effort and nocuous stimuli cannot, of course, give the same clear-cut answer as the specific galvanic reflex, but the tendency is in the same direction. When the painful stimulus is removed the muscular hypertonus relaxes slowly, and ultimately relief from pain is characterised by a far higher degree of relaxation than that found at the onset of the stimulus. This relaxation is well shown in Fig. 10. The pulse-rate becomes slower after the nocuous stimulus, and this slowing may be observed in the galvanometer curve of reaction to a pistol shot (Fig. 1). In this case, before the stimulus was administered, the pulse-rate had been more rapid than normal, and immediately after the stimulus was still further increased for a couple of beats and then becomes slower. We have here, therefore, a complex result of two different forms of excitation—the suggestion that a disagreeable experience is to be undergone, and the stimulus itself. It is characteristic of the cardiac response that its duration is almost confined to that of the stimulus itself; hence the

slowing that occurs after relief from the stimulus may be actually present while the other forms of response, such as the galvanic reflex, are still in progress. The response, therefore, to a benign stimulus is a diminution of organic activity. Philosophers of all ages have frequently drawn attention to the superior efficacy of pain as a vital stimulus. So far as an objective study of these reactions has taken us the reaction to a benign stimulus is only presented as a relief from the previous activity occasioned by a nocuous stimulus. It is not possible here to follow the line of thought suggested by these experimental results and to inquire whether benign stimuli with their pleasurable sensory aspects should not be considered merely as an interruption of the stream of nocuous impulses that spur the organism to activity. Theophrastes, quoting Anaxagoras, tells us that to feel is to suffer ἅπασαν δὰἵσθησιν μετὰ λύπης. If we substitute for the definite presentation of the specific sensation the vague affective state that accompanies a nocuous stimulus, the dictum of the early Greek philosopher holds true. Pain is the fundamental fact and pleasure nothing but relief from pain. It will be objected that moments of great joy are certainly accompanied by the exhibition of bodily activity—the winning side cheer and clap their hands—the dog will bound with pleasure when he recovers his lost master. This difficulty is easily dealt with. Such exhibitions of activity are not specific responses to the benign stimulus. They are not directed to the prolongation or the curtailment of the bodily reaction to the stimulus. They appear to be a compound of at least two factors, the discharge of certain reflex activities that have been inhibited by the tension of the muscular system, and the appearance of a specific type of nervous activity—the instinct to communicate with others.

A great deal of confusion has been occasioned by statements to the effect that the vasomotor reactions studied by the plethysmograph show an active dilator response to pleasurable stimuli which is in every sense the antithesis of the constrictor response to disagreeable stimuli. This statement, which is unfortunately copied from text-book to text-book, is based, as Shepherd has shown, on a very few experiments of Lehmann, which have not been confirmed by other observers. Using my own modification of the plethysmograph I have only succeeded in demonstrating the same type of slow return to the normal state from a vasoconstriction due to a nocuous stimulus that we have seen to occur with the galvanic response.

The objective study of these bodily responses to stimuli possessing feeling tone lends some support to the tridimensional theory of feeling of Wundt and the later somewhat similar view advocated by Royce. Without entering into any discussion of these questions it is still important for the understanding of neurotic disturbances to realise the plurality of the bodily responses that go to the make up of an affective state.

LECTURE III.

In the preceding lecture I dealt with some of the forms of response to nocuous stimuli from a physiological point of view. We cannot, however, content ourselves witb purely physiological observations. Our patients come to us complaining of certain dcfinite subjective symptoms and our object is to detect the defccts of bodily mechanism of which they are the signs. It therefore becomes a matter of urgent importance to determine so far as we can the relation between bodily responses to stimuli and affective states. It is true that we have learnt physiologically to observe only an infinitesimal part of tbe bodily changes that are associated with affective states ; but that small part is, as we bave seen, susceptible of quantitative as well as qualitative observation. As a first step we must consider briefly what we mean by an affective state and what relation it bears to sensation.

Sensation and Affective States.

Sensation is defined as an elementary mental process wbich is possessed of the attributes of quality, intensity, clearness, and duration. Affection is similarly an elementary mental process, but though possessed of the attributes of quality, intensity, and duration it has been denied that it possesses the attribute of clearness. It is said to be impossible to attend to an affection. If we attempt to do so the quality of pleasantness or unpleasantness disappears and we find ourselves contemplating some obtrusive sensation that we have no desire to observe. If we want to get pleasure from a concert or a picture we must attend to what we hear and what we see. As soon as we try to attend to the pleasure itself the pleasure is gone. (Titchener.) Herein lies the pith of the argument for dissociating affection or feeling from sensation. Tbe subsidiary argument that the opposition of the qualities of affective pleasantness and unpleasantness is not paralleled by any of the facts of sensation appears to me to be untrue, or at best unprovable, because we know no more about pure sensation than we do about pure feeling or affection.

It is at this point that objective psychology comes to the rescue. The reason for the apparent difference between affection and sensation has been made clear by neurological investigations, especially by the work of Head and Holmes. Two systems of afferent function exist—the one system reacts to stimuli of a nocuous nature, and thc effector organs which manifest its activity are either organs not under the power of conscious effort or clse certain combinations of reflexes which cannot be voluntarily activated. The great terminal ganglions for this mechanism are in the thalamus. This system is phylogenetically older than the cortical system. It originates the defensive reflexes by which the body reacts as a whole to nocuous stimuli, whether by glandular, visceral,

vascular, or muscular pattern reflexes. There is another system of nervous mechanisms by which appropriately adjusted reflex patterns are combined in response to stimuli not in themselves possessing either nocuous or benign characteristics, and the terminal coördinating centre of this mechanism is in the cortex. Its function is, to borrow the language of Head and Holmes, discriminatory, and this discriminatory adjustment of certain reflex mechanisms to various stimuli is correlated with consciousness.

The two systems are not, however, independent. When the activity of the cortical processes is impeded, by inhibition arising from either external or internal stimuli, an activation of the thalamic system may occur ; in other words, the resources of the body as a whole are mobilised to overcome the resistance. Conversely changes of the activity of the thalamic affector organs when they reach a certain degree of intensity stimulate the cortical receptors and we become aware of a change in the common sensation of the body. The consciousness of such a disturbance constitutes what we term an affective state. This reciprocity between the effector organs of the thalamic system and the cortical discriminatory system was already clearly appreciated by Hobbes—" adeo inter se motus cordis et cerebri sunt reciproci." The function of the cerebral cortex has been said to be discriminatory, a certain stimulus or group of stimuli being integrated with a specific reflex pattern activity, but when stimuli arising from the activity of the thalamic system reach the cortex they represent changes taking place in every part of the body. No one elementary group of sensations will be prepotent ; we are only aware of a diffuse sensory disturbance. Such appears to be the objective explanation of the origin of the attempt made by psychologists to differentiate between feeling and sensation. When the thalamic activity is of great intensity in response to a maximal stimulus, such as intense fear, certain of its elements may be so intense that they dwarf the remainder and are attended to as an ordinary sensation when their afferent impulses reach the cortex. Thus an absolutely definite sensation of nausea is experienced by some men when under fire, others complain of a burning sensation in the throat, others of palpitation ; and it is a peculiarity of the predominant sensations that they may so dwarf those arising from other forms of organic resonance that a man who is not conscious of any great degree of fear simply becomes aware of a sensation of nausea when under fire for the first time. It may not be till after he has gone through the experience on subsequent occasions that he recognises a connexion between his nausea and exposure to danger.

It is in the preponderance of some one such type of reaction, due either to innate or acquired auatomical abnormalities, that we find the explanation of so many of the visceral types of neuroses. In many cases the actual movement of the effector organ rather than its sensory concomitant holds the attention, as in the case of the well-known occurrence of vomiting, micturition, or defæcation as a response to nocuous

stimuli. In the last lecture we have seen that the organic response to nocuous stimuli is manifested as activity, and the response to benign stimuli as quiescence, and we are therefore justified in associating activity of the thalamic system with unpleasantness and quiescence with pleasantness. So far as it has been necessary to speak of these sensory concomitants of the bodily reactions I have spoken of them as states of affection or of feeling. From the physiological standpoint I can see no difference between an affective state and an emotion, and I am perfectly willing to use either term indifferently. From what can be gathered from the writings of psychologists it would appear that when certain sensory elements of an affective state sufficiently predominate so as to direct the attention overwhelmingly to their presentation the state is said to be one of emotion ; the difference is therefore one of quantity and not quality. I do not propose to enter into any attempt to differentiate the various types of emotions or affective states. Much of the psychological literature on this subject is characterised by that peculiar naïvete with which the scientific specialist too frequently tends to regard life. Our preceding short review of some of the objective changes verifiable as bodily responses to affective stimuli is sufficient to substantiate the acceptance of the popular psychology of " mixed feelings." We have seen that movements common to other states of activity may occur as concomitants of different affective states.

Problem of Origin of Affective States.

However much we may wish to confine our study of the neuroses to a purely objective basis we are unable to evade the question as to whether an affective state is to be regarded as conditioned by the physiological changes of bodily activity, or whether such changes occur as the organic resonance to a preceding affective state. Our whole system of the interpretation of the emotional aspects of the neuroses must be influenced by our answer to this problem. Ancient writers answered it in no uncertain fashion :

> " Cor ardet, pulmo loquitur, fel commovet iras,
> Splen ridere facit, cogit amare jecur."

Aristotle and his school of course recognised no other bodily mechanism for mental processes than that of the thoracic viscera. The Stoics, for the most part, rejected the brain as the seat of mind, and Zeno, Chrysippus, Diogenes, and Apollodorus were unanimous in proclaiming the heart to be the seat of the emotions. It would be too lengthy a task to follow the causal attribution of the affections to visceral changes throughout the Middle Ages. In recent times it has been revived in the James-Lange theory of emotion. I cannot do better than give the theory as propounded in James's original words : " Common sense says we lose our fortune, are sorry and weep ; we meet a bear, are frightened and run ; we are insulted by rivals, are angry and strike. The hypothesis here to be defended says that this order of sequence is incorrect : that the one mental state is

not immediately induced by the other, that the bodily manifestations must first be interposed between, and that the more rational statement is that we feel sorry because we cry, angry because we strike, afraid because we tremble, and not that we cry, strike, or tremble because we are sorry, angry, or fearful, as the case may be." This view has apparently been held in different forms by Sergi, Richet, Ribot, Münsterberg, and others, and has provoked a storm of controversy.

I shall not attempt to follow the arguments based on introspective considerations, but will turn to those furnished by objective methods. Sherrington attempted a physiological solution of the problem by depriving a dog, so far as possible, of all afferent paths of somatic sensation. He transected the spinal cord in the cervical region and subsequently performed double vagotomy. In spite of the deprivation of organic sensation that was occasioned by these measures the animal showed unmistakable signs of emotion when angered or menaced. The experiment is, as Sherrington recognises, by no means conclusive; all that it entitles us to say is that for the dog in this condition emotional manifestations were confined to the head and neck area, and we have already adduced evidence that changes in muscle tonus are just as much one of the factors of the organic emotional response as visceral and vaso-motor changes. Ward has urged that James's theory is a flagrant ὕστερον πρότερον—a putting of the cart before the horse. Emotion is always the expression of feeling; and feeling, for the subject that feels, has always some objective ground. Emotion is never the reception of impressions, but is always the response to them. Let us see how far a purely objective inquiry as determined by observations of the bodily reactions will help us to settle the question. Fig. 1 is the record of a reaction to a nocuous stimulus, manifested by the galvanic reflex, of a motor voluntary reaction and of an involuntary motor reaction or start. About the last-named reaction—the start—I have little to say; its latent period is very short, about 0·08 of a second. It can be, as Sherrington has shown, elicited as a purely mesencephalic reaction to auditory stimuli in decerebrate animals, when its latent period is about 50 σ. In this case the stimulus was a fainter one, and the latent period is appreciably longer than that found by Sherrington in animals— and this is the rule for any type of start reflex as elicited in man; it is therefore by no means certain that it has the same bulbar origin as the reflex in decerebrate animals. It is not an invariable accompaniment of affective states determined by nocuous stimuli, and for our present purpose we may disregard it. The next event recorded on the photograph is the voluntary motor reaction: the subject was instructed to press a key as soon as he perceived a prick. The latent period is here a normal one of 0·2 second. The third event is the movement of the galvanometer fibre at the beginning of the galvanic reflex. The latent period of this reflex is here 2 seconds. If the galvanic reflex be taken to indicate one of the many bodily responses that go to make up the general state of bodily activity

whose sensory equivalent is an affective state then this affective or emotional state is arrived at quite 1·8 seconds after the initial stimulus has been perceived and acted upon.

It might be urged that the galvanic reflex is a response sui generis with a remarkably long latent period, and that the other responses take place more rapidly. The analogy between the galvanic reflex

Fig. 1.

Galvanic reflex to prick stimulus. Record shows heart beats. Below is the record of the Deprez signal marked "key." Time above in fifths of a second.

and the time relations of the " cat pad " phenomenon of Luchsinger might prima facie lead us to the same conclusion. The experiment of Luchsinger, it will be remembered, consisted in observing a secretory current from the pad of a cat's foot when the sciatic nerve was stimulated electrically and the latent time of this reaction, measured by Waller in the human subject, is about 2 seconds. The latent period of

the plethysmographic response can be measured with a fair degree of accuracy, and in the previous lecture we have seen that its time relations are the same as those of the galvanic reflex. The latency of the alterations of pulse rhythm cannot obviously be determined accurately, but by collation of a large number of tracings I have never found it to be a shorter period than 2 seconds. We have seen reason to believe that the acceleration of the cardiac rhythm is due to removal of the inhibitory action of the vagus, and physiological experiments assign a latency of about 1 second to the vagal reflex.

The latent period of the respiratory changes is similarly incapable of accurate determination, but I have never found it to be less than 1 second. The muscular tonic response in the majority of my determinations had a latent period of about 2 seconds. I have no data as to the latent period of visceral and glandular activity, but indirect evidence would point to their being at least equally long. Thus the objective evidence is unequivocal—the stimulus is perceived, differentiated, and acted upon a considerable time before the presentation of the sensory concomitants of the bodily reactions could be experienced as an affective state. In my own case introspection seems to confirm the objective data. Experiences in the line in France were perhaps a little too complex for analysis, but in the streets of London my absence of mind has frequently nearly involved me in a motor accident. In such cases after successfully avoiding the oncoming vehicle I have experienced some 2 seconds later the feeling that we used to call as children "going hot all over." Had the stimulus been less transitory and the motor pursued me instead of avoiding me the continued organic response would on the sensory side have amounted to what we know as a state of fear.

Possibility of Existence of an Objectless Affective State.

It is of great importance for the understanding of the nature of neuroses that we should determine whether a reversal of this normal sequence of events can take place ; that is, can a pre-existing state of bodily activity, which is the physical aspect of an affective state, determine the orientation of cortical activity so that the state of emotion arises first, and then an object is assigned to it ? It may be disputed whether an objectless affective state can exist, but there is ample evidence that the existence of the bodily concomitants of an affective state will facilitate the representation of some situation which will then be assigned as its object. Marañon injected a small dose of adrenalin into a sensitive patient, and obtained various subjective sensations which as a rule constitute the syndrome of terror. At times this was noted by the patient himself, who declared : "I feel as if I were afraid, but I am calm." When, however, he supplied an appropriate verbal stimulus by mentioning some source of anxiety to the patient, although such a stimulus would naturally have caused but little

reaction under normal conditions, the patient would break down completely in a fit of emotion.

Hitherto I have not dealt with the influence that may be exercised on the response of the nervous system through the agency of the internal secretions.

Disturbance of Endocrine Secretions in Response to Nocuous Stimuli.

The classical experiments of Cannon seem to have demonstrated that states of excitement are accompanied by a hypersecretion of adrenalin, as evidenced both by direct serum tests for its presence and by the occurrence of glycosuria. His brilliant demonstration has been received with great enthusiasm and a far greater rôle has been assigned by the uncritical to the part played by the hormones in the activity of the nervous system than can be justified by observation. It is too readily forgotten that the effector organs of the vegetative nervous system respond to the internal secretions of the adrenals in precisely the same way as they would if acted upon by the vegetative system directly. It is therefore no proof that a reaction is due to the stimulation by an internal secretion, and not to the autogenous activity of the nervous system, to say that the reaction resembles that which is experimentally produced by the injection of the hormone. Swale Vincent has pointed out that there is no evidence whatsoever that adrenalin, for example, is a normal constituent of the blood ; indeed, there is much cogent evidence against such an argument.

That under certain conditions of great excitement adrenalin may be present in the blood seems to be fairly well established from the work of Cannon, but were it secreted as a response to stimuli of a less intense and prolonged nature it could only be present in the blood in such minute amounts as to be incapable of initiating the bodily emotional responses. I have conducted a series of observations on my own blood taken before and after the experience of ten minutes' faradisation. In no case was I able to detect any hyperglycæmia as a reaction to the pain stimulus. It is obvious that the painful stimulus was inadequate inasmuch as it neither increased the blood sugar nor produced glycosuria, but the pain was certainly more severe than is likely to occur in the normal experience of healthy life, and it seems legitimate to assume that the ordinary nocuous stimuli of daily life are unlikely to affect the secretion of adrenalin. This criticism applies even more strongly to the internal secretions of the other endocrine organs as a direct response to nocuous stimuli. When we turn to the evidence in support of the direct influence of the internal secretions on the threshold excitability of the thalamic system, we are on surer ground. There is ample clinical evidence of the state of hyperexcitability of patients suffering from hyperthyroidism to stimuli adequate to arouse an affective response. Experimental observations show that the hormone of the thyroid has a marked influence on bodily activity. This hormone has been isolated and identified by Kendal as an indoxyl

iodine compound and found by him to raise the rate of basal metabolism 2 per cent. in a man weighing 70 kilos. when administered in a dose of 5 mg.

I have in a previous lecture dealt with the diurnal variation of the bodily resistance described by Waller, and I showed that there was evidence of a rough correspondence between the magnitude of the galvanic response and the diurnal resistance variation. In order to test the action of ingested thyroid gland on the galvanic response I again made use of this diurnal variation. By taking large doses of thyroid in the early morning I was able both to diminish the mean value of the skin resistance and to mask its diurnal variation. At the same time the galvanic response to uniform sound stimuli of moderate intensity was markedly increased. Fig. 2 may be compared with the record of the diurnal variation of resistance and the magnitude of the galvanic response represented

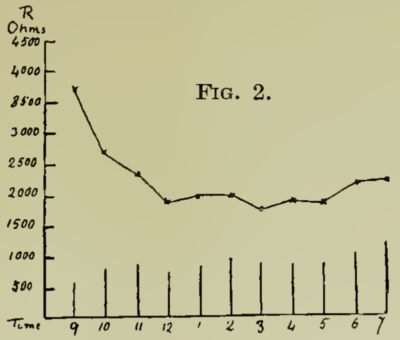


FIG. 2.

Above, curve of diurnal resistance as altered by ingestion of thyroid extract—compare Fig. 8, Lecture II. Below, responses × 2 to maximal stimulus (pistol shot).

by Fig. 8, in the second lecture. Each diagram represents the mean observations of three successive days. Subjectively, whilst under the influence of thyroid in large doses, I was aware of a marked degree of irritability which lasted throughout the time of the thyroid ingestion experiments, but which appeared to be accompanied by a rather increased power of doing work. I give these subjective observations for what they are worth. That a diminished skin resistance occurs in cases of exophthalmic goitre was shown long ago by Vigouroux. The action of adrenalin in producing effective states has already been considered ; but in the absence of any proof of its normal presence in the blood stream we have no right to consider that the normal activity of the affective thalamic mechanism is influenced thereby. There is some direct evidence that the internal secretions of the gonads facilitates the specific affective response to sexual stimuli as shown by the results obtained with implantation of testicular grafts in man and animals suffering from loss of sexual desire. Although there is every reason to believe that the internal secretions of other glands have a direct influence on the affective responses

it is difficult in the present state of our knowledge to bring forward any incontrovertible proofs of this hypothesis. It must never be forgotten that a glandular structure in the normal body is not inert so far as the furnishing of afferent impulses to the central nervous system is concerned.

It may well be that these afferent impulses and not the internal secretions evoked by their activity are the determining influences of the glands on any particular mechanism of response. I was able to obtain records of the secretion of saliva from my own parotid gland by applying a small catheter tube maintained in position by a suction ring over the orifice of the duct. This procedure has been adopted by other observers for the recording of conditioned reflexes produced by the sight of food ; I found that nocuous stimuli, if of sufficient intensity, tended to produce inhibition of the saliva flow. The fall of the drops was recorded by allowing them to tilt a tipover key that closed a Desprez signal circuit and thereby recorded on a smoked drum. There is not wanting evidence of the disturbances of the gastric, intestinal, and renal secretions as a response to nocuous stimuli. We have so far seen that the mechanism of all forms of cerebral activity is capable of being recorded in terms of muscular and glandular activity ; before attempting to transfer this knowledge to the investigation of neuroses we must consider the constitution of some of the more complicated nervous mechanisms in their barest outlines. In the last lecture I showed a slide (Fig. 7) illustrating a conditioned reflex ; you may perhaps remember that the subject was taught by means of a painful stimulus to perform a certain movement and that we noted that when the stimulus was intermitted, whilst the audible sign that had hitherto accompanied it was made, the subject responded precisely as if he had perceived the stimulus. We also noted that the signs of an affective response, as evidenced by the galvanometer, were present when the click of the electric key occurred in the absence of the stimulus. In the language of popular psychology the subject had formed a habit, or in that of physiology a pattern reflex had been conditioned. Now supposing the stimulus had been repeated a requisite number of times, the subject would, at any rate for the first few occasions on which the click of the key was alone given, have found himself utterly unable to control the movement of pressing the signal key. But this movement was in the first instance one that had been acquired as a result of the activity of the discriminatory mechanism of the cortex. It has now ceased to be discriminatory and become independent of the first process of nice adjustment of a specific mechanism to a specific response by the inhibition and coördination of other forms of cortical activity. There is, however, no evidence that the response has ceased to be controlled by the same cortical mechanism which started it. I found in this particular instance that after the first few times the voluntary muscular reaction settled down to a constant latent period of 0·18 second and that later when the conditioned reflex was elicited by a click alone the latent period

remained unchanged. Owing to the slow rate at which these particular records were taken the latent periods could not be read closer than to 0·02 second, but the accuracy is sufficient to justify our assumption that the reaction continued to be performed by the same cortical mechanism that had been responsible for its initial manifestation.

Now this statement embodies a very important fact of which the psychotherapist endeavours to make full use. It means that when the reaction had become part of a conditioned reflex and had ceased to be the centre of conflicts between various alternative reflex paths, which conflicts we generally associate with conscious perception, the conditioned reflex still made use of the same cortical mechanism. That is, it still had a direct nexus with all those processes with which it had been originally connected. Therefore by the use of appropriate methods of observation it should be possible to determine what the original associations were when they are no longer objects for the subject's own consciousness.

Conditioned Reflexes in Daily Life.

Now the greater part of our daily conduct is made up of such conditioned reflexes or habits, and the more effectively they have been established by constant repetition the more difficult it is to interrupt the reflex pattern by some fresh stimulus. We defined one of the attributes of a nocuous stimulus as a tendency to interfere with and impede the normal tendencies of vital activity. There is no more fixed or powerful activity than that of a well-established habit mechanism. A verbal or physical stimulus that impedes the activity of a habit reflex pattern will therefore act as an intense nocuous stimulus, and through the inevitable thalamic mobilisation of the bodily activities to resist such interruptions an intense affective state will be the sensory result. Not only stimuli from outside may tend to impede the activity of the habit mechanisms. Attempts on the part of the subject to interfere with them will equally evoke the generalised bodily response with the corresponding unpleasant feelings. Lastly, the constant occurrence of these reactions, whenever the habit mechanism is interfered with, may in itself condition a new reflex pattern—an active opposition to all attempts to cause the feeling of discomfort by interfering with the primary habit. Such, I believe, is a restatement in the language of objective psychology of the rather clumsy symbolism which the psycho-analyst makes use of; such terms as "the censor" would appear to be unnecessary in this connexion. There is another type of pattern reflex even more potent and more dissociated from the discriminatory mechanism of the cerebral cortex. Unlike the conditioned reflex it has never arisen in the life of the individual from a discriminatory response to a stimulus. This mechanism is an inherited one, and is unchained by some one specific type of stimulus and by no other. Such innate reflexes of a specific type are in greater or less degree common to all the individuals of the

same species; these inherited reflex patterns are known as instincts.

Instinct Reflexes.

Now by definition an instinct is an inherited pattern reflex. It is evoked by a specific stimulus and in response to this stimulus the pattern is unfolded. The end-result of the reflex is implicitly determined by the nature of its innate mechanism, and it is a mere matter of chance whether this end-result is ever presented to the subject who owns the reflex. Though by observation we may sometimes learn what the end-result really is, it by no means follows that we shall necessarily do so, and in the case of the lower animals—as when an insect, after laying its eggs, prepares food for larvæ that it will never live to see—it is quite certain that it can never know the end-result towards which the instinct reflex is orientated. Now it is of great importance to the understanding and treatment of the neuroses that we should know whether an instinct reflex has any such relation with other discriminatory mechanisms as may allow us by association methods to determine its true orientation. We may recognise that a certain line of conduct is not substantially in harmony with the environment, that attempts to impede it cause great emotional disturbance, that its origin is obscure and its end-result opposed to the interests of the patient. It is, however, only by tracing some affinity between its mechanism and the nervous system as a whole that we can hope to understand or influence it. In the case of a habit or conditioned reflex this is relatively easy. It arose as a discriminatory response to a certain stimulus—that is, it was once an object of experience and may again be recalled by appropriate methods. But if the instinct be an inherited mechanism, how can its origin be with any certainty deduced from its relation to other mechanisms accessible to consciousness? The problem is a fundamental one, and has presented itself in many forms.

To the Atomists, to Heraclitus, and to the Pythagoreans (who never drew a distinction between animal and human intelligence) the problem was never presented, nor do we see any trace of it in the anthropomorphic animal psychology of Plutarch. The conception of a divergence between abstract thought and animal life is implicit in Plato, and the distinction became more definite in the writings of Aristotle and the Stoics. In scholastic thought we find instinct formally recognised as comprehending the totality of animal behaviour and the baser part of human conduct, whilst in Neo-Platonism there is a reversion to the standpoint of the Epicureans. With the wane of scholastic thought the tendency to the anthropomorphic view of animal behaviour has persisted till the present day. Darwin formulated our present knowledge of instinctive behaviour and gave full recognition to its recurrence in man and its evolutionary significance.

The problem as to the relation of instinct and intelligence now becomes a biological one. Those who hold the Neo-Lamarckian doctrine as to the

hereditary of acquired characters may consider that instinct is nothing else than a hereditary conditioned reflex—that somewhere in the past a discriminatory action became a conditioned reflex, and by the aid of natural selection such a conditioned reflex has been handed down as an innate part of the nervous mechanism. If an instinct in its earliest stages originated as an act of conscious discrimination its later hereditary form will still show some affinity to the conscious mechanism that first started it, and we might hope that by learning something of its cortical connexions we would arrive at its origin. On the other hand, if an instinct be regarded as a variation preserved by process of natural selection, it is clear that to seek its early cerebral connexion is futile, and if we disbelieve in the heredity of acquired characters the instinct reflex pattern must be unfolded de novo in each generation, and the associations that it may possess will only be such as may have been acquired during the life of the individual. Men of such different schools of thought as Ziegler, Haeckel, Spencer, Wundt, Semon, Ward, McDougall, and Preyer, adopt the Lamarckian standpoint; Driesch and notably Bergson represent a return to the scholastic view of instinct. For Bergson instinctive behaviour represents a form of activity to which there can never be a psychical correlation in the terms of human practical intelligence. Human intelligence is so developed and so limited as to enable us to adopt mechanical aids to life, and therefore postulates at the beginning of any action a more or less clearly defined conception of what its end is to be. Instinctive behaviour knows nothing of ends; it can only reply to introspection in the words of Luther, "Ich kann nicht anders."

I venture to think that all speculations about the psychical aspects of instinctive behaviour founded on animal observation are of very doubtful value. The supreme act of faith is the attribution of consciousness to our fellow beings, for knowledge of consciousness is never immediately given; but to proceed to speculate about the psychical correlations of instinctive behaviour in insects cau hardly be helpful to objective psychology. We must turn to human instincts, and we must obviously select for study of instinctive behaviour an act performed for the first time and from which mimicry and verbal suggestion are excluded.

Expression of the Animal Nest-making Instinct in Human Beings.

Some years ago I observed what has always seemed to me to be a singularly interesting and beautiful instance of instinctive behaviour that fulfils these postulates. If it has been already noted by others I must apologise for presenting it under the guise of a new observation. A primapara about two days before the birth of her child worked very hard at tidying not only all the drawers of her bedroom and boudoir but also her husband's desk. Asked why she did this she said that as she was going to be ill for

a long time she wished to have all her own and her husband's things in perfect order. The answer seemed to me to be somewhat inadequate, since as a rule the husband's desk was never interfered with. Some time later the father of several children remarked to me that he always knew when a birth was due, since for a couple of days before it his wife in an excess of energy would tidy every drawer she could find including all his private papers. When I asked him why she did so he said that she gave him the same explanation as I had received in the first case. Now I knew this lady sufficiently well to feel sure that on the occasion of her long absences at her country house, where she spent many months every year, the last thing that would occur to her would be to prepare for her absence by such a minute ransacking of drawers that are as a rule outside her province. I then asked a midwife if this procedure was a usual precursor of childbirth. She said that she had observed it in quite the majority of her cases, and inasmuch as her practice lay among some of the wealthiest people of the country in whose houses such duties as the arrangement of drawers and cupboards are seldom performed by the mistress the phenomenon was all the more striking. Later on when observing the behaviour of a doe rabbit in a breeding hutch I noted that for three or four days before delivery the doe spent her time in a state of frantic activity collecting bits of hay, tossing them up to unravel them, and finally carrying them to the dark compartment at the end of the cage. She was making a warm nest for the expected young. When we find that this nest-making behaviour is universal among the higher apes I think that we are entitled to assume that the drawer-cleaning, sorting, and tidying are the expressions of the animal nest-making instinct.

We have here an instinct untainted by any possibility of mimicry, and since its existence is apparently unknown, free from any complication due to suggestion. The purpose of the reflex instinct mechanism is unknown to the expectant mother, and when her attention is directed to it, and she is asked what she is doing, she casts about for an explanation and evolves one that is not only false but shows no associational nexus to the true purpose of her activity. The distinguishing feature of an instinctive reflex is its compelling urgency. The least delay or opposition to its accomplishment, once it has been aroused by the appropriate stimulus, evokes an intense bodily reaction, and it would appear to be of the imminence of such an explosion of feeling that we speak of as a feeling of compulsion. This feeling of compulsion is the real equivalent of what Bergson terms intuition in respect to instinctive activity. Like the Bergsonian intuition, it cannot be translated into the language of practical thought, nor has it association of a direct nature with aught other than the immediate instinct mechanism. These innate pattern reflexes that we term instincts undoubtedly play an enormous part in the mechanism of conduct. Their recognition in all but such simple cases as I have just now narrated is a matter of great difficulty. They can as a rule be

only distinguished from the conditioned habit reflexes by a minute knowledge of the subject's past history such as neither he nor the observer can hope to possess. Much of the vast output of writings on the subject of the instincts that has been characteristic of the last few years loses considerably in value on this account. As I am concerned only with an attempt to show what fundamental knowledge concerning organic disturbances of mechanism can be gained from an objective study of bodily responses and of conduct, I will make no attempt to deal with any system of classification of the different forms of instinctive mechanism. There appears to be a tendency to over elaboration in this direction—Essentia non sunt multiplicanda praeter necessitatem—the "razor" the "doctor invincibilis" has its application in psychological medicine as well as in philosophy. One particularly deplorable move in this direction has been the invention of states or attitudes to take over the very attributes without which an instinct cannot be recognised as such. Thus to account for the compelling power of the instinctive mechanism that will drive a man into courses of conduct that are directly at variance with his interests in society as at present constituted, we are invited to divest the instinct of its compelling power and to lay it at the feet of an entity termed the "self regarding sentiment" for whose satisfaction we yield to the unprofitable enterprise. There is a flavour of mid-Victorian self-righteousness about this sentiment; it may possibly have been of great usefulness in strengthening the resolutions of conscientious objectors, but the men who sacrificed their interests in the war because they felt that they could not do otherwise had little use for it. The modifications that will be imposed on a primary instinctive mechanism by the pressure of the environment will be incalculable. It is for this reason that in the elucidation of both instinctive reflexes and conditioned habit reflexes their affinities to the affective states are likely to afford a truer if less detailed insight into their nature than any attempt to range them in a tentative scheme of classification.

From what has been said of the relations of bodily response to nocuous and benign stimuli it is obvious that I must consistently range the processes of activity that are distinguished as appetites as being nothing other than responses to nocuous stimuli. From the objective point of view such activities are the response to the present situation of pain and not the pursuit of pleasure. The satisfied man can have no appetites. The nocuous stimulus that arouses our activities is not that conditioned by the desired object itself, but by the limitations imposed by the environment that impede the possibility of our satisfaction by its possession. What is true of appetite is true of desire. As Spinoza remarks, there is no difference between appetite and desire except in so far as the latter implies consciousness. Desire is self-conscious appetite.

LECTURE IV.

In the preceding lectures I have endeavoured to give some account of the objective features of the fundamental mechanisms of cerebral activity. I propose in this concluding lecture to indicate in what fashion objective psychology may be applied to the study of the neuroses. We have seen that the mechanisms of cerebral activity are fundamentally of two characters. In the first place the discriminatory cortical activity finds its expression in the movements of the voluntary nervous system—that is, in actual muscular movement and in the kinæsthetic representation of such movements in the motor cells of the cortex. In the second place the general reactions of the organism to stimuli that tend to destroy or impede functional activity find their expression in the movements of the thalamic system and effector organs. Since the nervous system is an instrument of movement, and nothing else, we should be prepared to find that a certain group of the functional disturbances that we term neuroses would be the expression of some chemico-physical changes affecting movement of the neurones generally. Our knowledge of the chemico-physical processes that constitute activity of the neurones is as yet too incomplete to furnish a secure basis for a descriptive pathology.

The Future Task of Neurology.

At present we rely chiefly on deductions from the similarity to certain aspects of neurosis in the behaviour of the organism when functional activity is disturbed by toxic substances, whether ingested or manufactured, in the body. The study of these physico-chemical disturbances will be the great task of the neurology of the future. The researches of Sir Frederick Mott and his pupils have broken new ground in the elucidation of the micro-chemical signs of disturbances of the function of the neurones, whilst the work of Pighini has opened the path for chemical investigation through the study of metabolic disturbances. The policy of the London County Council in giving effect to the scheme endowed by the late Dr. Maudsley in founding a school and hospital for the study of the earliest stages of nervous disturbances is justifying itself by the additions now being made to our knowledge of the chemical and physical changes that express disordered function of the neurones. The work of Koch and Sydney Mann performed in the L.C.C. laboratory suggests that the general bodily deficiency of oxidation activity demonstrated in dementia præcox is the interpretation of the micro-chemical changes shown by Mott to be characteristic of the nerve cells in this disorder. Just as such an advance in our knowledge of the physical basis of this form of insanity must once and for all dispose of the theories of its psychogenic origin such as that advanced by Jung, so we may hope that by similar

methods the physical basis of the asthenic forms of neuroses may be established.

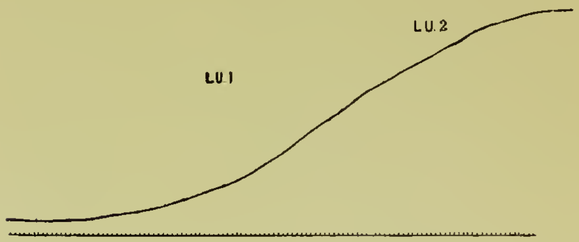
There is, indeed, a certain body of physiological evidence in favour of the view suggested by the micro-chemical methods of Mott and his pupils, that the processes of nervous activity are of the nature of a catalytic phenomenon. Brailsford Robertson repeated an earlier observation by Loeb and Koryanyi on the determination of the curve of the velocity with which a simple movement of drawing a straight line of indeterminate length is performed. Robertson ruled straight lines with a form of apparatus allowing the construction of a curve of the velocity with which his pencil travelled at any moment of the drawing. The pencil, which was in reality a metal stylus connected with one arm of the circuit of a battery activating a Deprez signal, travelled across a number of longitudinally arranged alternating slips of ebonite and copper. The copper slips were each connected with the other arm of the signal circuit. When the stylus travelled over one of the copper slips the circuit was closed and the signal made a mark on the smoked paper of a drum rotating at uniform speed. Since the copper slips were equidistant it is obvious that by measuring the distances between the signal marks on the drum it was possible to ascertain the velocity with which the stylus moved at any point of its journey. These data furnished a curve of the velocity which showed the following characteristics: The velocity underwent progressive acceleration; beginning slowly, the velocity of the stylus reached a maximum and then again diminished, so that the commencement and termination of every curve was asymptotic.

In order to verify the conclusions of Robertson I arranged an endless band of linen revolving with constant velocity round two drums set horizontally at a distance of about two feet apart. The band of linen was a yard in width. Midway between the two drums it passed over a third roller. The apparatus was covered by a board except for a gap about 2 inches wide just over the middle roller. A line of arbitrary length was ruled with a pencil along one edge of the gap and the quickly moving band did not allow the curve traced to be followed by the eye. No attempt was made to determine voluntarily either the velocity with which the line was drawn, or its length, so long as it was within the limits of the band. Lines were drawn both towards and away from the body, using movements of the whole arm, elbow-joint, or wrist in order to eliminate the possible influence of the type of movement employed on the form of the curve. When the band was removed the resulting curves in every case agreed with that constructed by Robertson (Fig. 1). Now Robertson has shown that such a curve is typical of an autocatalytic process—that is, a chemical process of such a nature that it is catalysed by one of the products of reaction; in such a case the commencement and termination of the velocity curve is asymptotic.

Examples in chemistry of such autocatalytic reactions, in which one or more of the products of reaction

accelerate their progress, are furnished by the hydration of methyl acetate or the hydrolysis of cane sugar at 100°. In such a reaction a point must be reached when the decrease in the active mass of the reacting substance balances the increase of the active mass of the catalyser. A catenary reaction is the only other type of reaction that will show a similar acceleration, but the autocatalytic reaction is distinguished

FIG. 1.



Curve traced by drawing a straight line across a rapidly-moving linen band. Time below in 1/100 second.

by the fact that its curve is symmetrical. The integrated equation for such a curve will be—

$$\text{Log} \frac{x}{A-x} = K(t-t');$$

where A expresses the total transformation at completion of the process, K is a constant expressing the velocity of the process, and t' is the time at which the mass of the product equals $\frac{1}{2}$ A.

Correlation of Catalytic Reaction with Defective Oxidation in Nerve Cells.

Now the importance of this observation lies in the evidence that it affords that the neural process conditioning the movement is in the nature of a catalytic reaction. The researches of Mott have shown that the nervous asthenia of extreme forms of myxœdema and of dementia præcox are associated with evidences of defective oxidation processes in the nerve cells, and the correlation of the two observations affords an alluring hypothesis. The casual drawing of a straight line affords a very good example of a cortical mechanism, unchained in response to an autogenous voluntary stimulus, which runs its course without further interference. We may look to observations of disturbances of the catalytic process such as those furnished by Koch and Mann in dementia præcox to afford an insight into the asthenic types of neurosis. The slide shows the rapid exhaustion of a patient who suffered from lack of power of concentration and a distressing sense of mental fatigue when he was set to obtain a record with the Mosso ergograph. That the fatigue was not peripheral was shown by the absence of signs of muscular fatigue in response to electrical stimulation before and after the ergographic tracing. By correlating my method for the estimation

of the effort tonus reflex, described in the first lecture, with observations on the squeeze exerted on a dynamometer I have noted that in such patients the bodily response of effort dies down rapidly, and simultaneously with the maintenance of the squeeze. The effort is, however, there, and this servcs to differentiate this class of patients from those that we shall consider later under the category of hysterical weakness. It is in the treatment of these asthenic patients that so much harm is wrought by attempts to stimulate them to greater efficiency without first restoring their bodily condition.

Necessity of Constitutional Treatment of Asthenic Patients.

All neurologists of experience are convinccd of the great efficacy of constitutional treatment which is too often ignored by the new school of psychotherapy. I will now turn to consider those neuroses in which the predominating symptoms are manifested objectively and subjcctively as an excessive response on the part of the mechanism that we have studied as the bodily reaction to nocuous stimuli. I have endeavoured to show that the bodily response to nocuous stimuli is essentially one of activity, and that the benign or agreeable stimuli are responded to by quiescence ; and, furthermore, that the activity which we occasionally ascribe to the action of agreeable stimuli is really the response to those conditions of the environment that hinder their enjoyment. The time at my disposal would not allow me to attempt to consider in detail the various forms in which such a hyperexcitability of the mechanism of affection may show itself.

In the previous lecture I spoke of the caution with which we must view all attempts to attribute activity of the vegetativc nervous system to the direct action of the internal secretions ; it is much more promising to seek for evidence of modifications of the excitability of the autonomic mechanism as a result of long-standing hyper- or hypo-function of the endocrine system. The rclations between excitability of the autonomic system and glandular activity havc been more extensively studied in the case of the thyroid than in that of any other gland ; I will not dwcll on the well-known symptoms of hyperthyroidism ; there are, however, a ccrtain number of cases of neurosis exhibiting a hyperexcitability of the affective mechanism together with tachycardia, vasomotor disturbances, and tremor without any thyroid enlargement. Therc are three signs revealed by the objective examination of such cases on which I lay great stress. First, the hyperexcitability of the mechanism of affection as revealed by a study of the galvanic reflex ; in these cases the skin resistance is invariably very low. This sign, though never absent in indubitable cases of hyperthyroidism, can, of course, taken alone, only amount to an objective demonstration of the hyperexcitability of the affective mechanism. The isolation of thyroxin, the active principle of the thyroid internal secretion, has permitted the quantitative

estimation of the influence of the thyroid on metabolic activity. Since in cases of indubitable hyperthyroidism we invariably find the basal metabolism to be above the normal rate we cannot diagnose its presence in the absence of this sign. The determination of the rate of basal metabolism is a procedure of no great difficulty, which can be accomplished in a very short time with fairly simple apparatus. It is to be hoped that this valuable method will soon become an established procedure in the investigation of cases of neurosis. There is, however, a third sign of the existence of the condition of hyperthyroidism which is likely to prove of great value. Goetsch has devised an intradermic test that serves to demonstrate the existence of hyperexcitability of the sympathetic system. The reaction depends on the demonstration of sympathetic hyperexcitability by injection into the superficial layers of the epidermis of minute quantities of adrenalin. The test shows no more than that the sympathetic system is hyperexcitable, and furthermore it only demonstrates a hyperexcitability at the junction between the sympathetic neurone and the effector cell. It cannot, therefore, be held to be of absolute diagnostic import, any more than the exaltation of the galvanic reflex. Its importance lies in the fact that it is apparently never absent in cases of hyperthyroidism, and taken with the two preceding tests it will establish the diagnosis. As modified by Ascoli and Faggioli, the test is described thus : When 0·05 c.cm. of a 1 per 1000 solution of adrenalin is injected by a fine hypodermic needle, pushed just under the epidermis, so that its whole intradermic course is clearly visible through the skin, the resultant swelling assumes after a few minutes a dark-blue colour as if ink had been injected. The swelling then becomes surrounded by an alabaster-like halo which grows in intensity and extent, often sending out irregular shoots in one or more directions. Around the white halo appears in turn a red halo, more or less intense in colour and width. Frequently a contraction of the pilomotor muscles occurs, giving an appearance like goose flesh to the alabaster zone. This is fairly well shown by the photograph (Fig. 2). The area of goose flesh in my photograph is not, however, limited to the alabaster zone. Having reached its maximal development in half an hour, after a stationary period of an hour or so, the reaction gradually disappears, the blue spot changing to red, and leaving ultimately a small swollen papule. By using a more dilute solution, for instance, one in two hundred thousand to one in a million, the reaction is less intense but always preserves the same type. It differs, however, by the absence of the central blue spot for which a small red mark is generally substituted later on, and the red halo is not always prominent. The excitability of the subepidermal test in ordinary circumstances lies between dilutions of one in two hundred thousand and one in a million (Parke Davis phials). In some cases of disturbances of the menopause, of arterial hypertension, of Graves's disease, and sometimes in pregnancy, the excitability is increased, giving positive results with further

dilutions up to one in five to twenty millions. It should be always compared with a control injection of distilled water, as some people react in a somewhat similar fashion to the water injection.

Using this technique in preference to that of Goetsch I have obtained positive results in all those of my cases exhibiting indubitable symptoms of hyperthyroidism with hyperemotionalism, and in some dozen cases occurring at the climacteric in which flushing, giddiness, and nervous tremors were complained of, and which showed an excessive galvanic reaction. So far the evidence seems to be clear that hyperactivity of the mechanism of affection is associated with the frequent occurrence of hyperactivity

FIG. 2.

Intradermic test for sympathetic hyperexcitability, showing the needle track and an appearance like goose flesh in the alabaster zone.

of the thyroid. That thyroidism is not the only cause is certain; we find such hyperexcitability associated with other bodily states, in diabetes, at puberty, at the climacteric; it can be produced artificially by many drugs such as adrenalin. That hyperthyroidism can be conditioned by the environment is certain; cases occurred during the war in which pronounced symptoms of Graves's disease were noted within a few days of exposure to the strain of action; but I have already sufficiently emphasised in my first lecture that considering the multitudes subjected to like conditions we cannot but come to the conclusion that

the very small number of men affected had a pre-existing abnormality of the thyroid function. There is good reason to believe that a long-continued excitation of the affective mechanism may lead to a temporary hypertrophy of the thyroid activity; this fact was demonstrated by French observers in the civilian population of bombarded towns, but where there is no pre-existing state of organic instability such states are not lasting. The implantation experiments of Steinach have demonstrated that excitability of the affective mechanism subserving sexual activity may be directly influenced by the internal secretions of the gonads.

Problem of Relationship of Internal Secretions to Nervous Mechanisms.

Our knowledge of the relationship of other internal secretions to nervous mechanisms is as yet too scanty for discussion; such secretory activity may well be the determining factor in unloosing innate types of nervous mechanism or instincts—e.g., the unchaining of the dormant maternal instincts in the capon. Some of the wildest imaginings of the novelist may, indeed, have a basis in physiological fact. It is by no means inconceivable that latent innate reflex patterns may occasionally be aroused without any form of suggestion by knowledge or example on exposing the individual to the appropriate environmental stimulus; examples are not wanting in the study of animal behaviour. The mention of these abnormalities of instinctive behaviour leads us to the consideration of another type of neuroses, in which the distinguishing feature is some line of conduct, which is at entire variance with the demands of the environment and which we can recognise to be related to the affective symptoms that are aroused by any interference with it. We have already seen that it is hopeless to seek in such conditions for associations between the primitive innate mechanism and conscious discriminatory processes. These types of instinctive behaviour may occasionally have been re-awakened by the environment, or they may owe their appearance to certain structural abnormalities which constituted the primary stimulus at a period when the innate mechanism was not at variance with the more primitive conditions under which the ancestral stock lived. It is only the manifestation of primitive instincts that are ill-adapted to the environment which is of importance to neurological medicine. Similar in their manifestations to the instincts, and yet so different in their origin, are the conditioned reflex patterns or habits. Both types have, as we have seen, this in common, that interference with their free activity is responded to by the nervous system in the same fashion in which it responds to a nocuous stimulus which tends to injure or impede the normal course of vital activity. The resultant powerful bodily reaction with its disagreeable affect may on frequent repetition condition a new defensive habit mechanism of which certain writers who prefer the

terminology of dynamic symbolism speak of as repression by a censor whose habitat is the unconscious. In normal people, whose mechanism of affective reaction is not hypertrophied, such unfitting instinct and habit mechanisms will cause but little disturbance. In those whose affective mechanism is hyperexcitable the disturbance produced may be so profound as to affect every manifestation of conduct. What is in some men an eccentricity constitutes in others a neurosis.

Problem of Stimuli Originating Habits.

Now though to endeavour to discover what are the primary psychical correlatives of an instinct mechanism is to attempt the impossible, to determine the form of stimulus that originally conditioned a habit is a more hopeful task ; by doing so we may be able to introduce the links of the habit mechanism to other and more rational associations and so to disintegrate its power. It is this task that is attempted by those who try to arrive at the original condition by the analytic method. I do not propose to discuss the utility of the psycho-analytic method. That it has given much valuable information in the realm of psychology is beyond question ; whether its application in the field of medicine has yielded advantages outweighing its manifest drawbacks is a question about which there is much difference of opinion. An attempt to discover the associations presumably existing between different processes of cerebral activity and a particular disordered mechanism by learning from the patient what he can tell of the associations which are most readily evoked by an appropriate verbal stimulus is of course a perfectly legitimate objective method, but in an attempt to base our study of neurosis on purely objective investigations we are bound to scrutinise very carefully the validity of the means which we adopt.

Difficulties of Inquiry by Method of Association.

There are several factors involved in an inquiry conducted by the method of association that must prove to be very grave difficulties in the path of anyone seeking to conduct investigations in the spirit of objective science. These difficulties are probably quite well realised by the many acute intellects that have busied themselves with the analytic method, but to recognise a difficulty is not always to be able to avoid it. The chief source of error in such investigations must always be the possibility that the subject derives his associations not from endogenous sources but from the investigator. The most casual perusal of the literature of psycho-analysis must convince any unprejudiced observer of the reality of this difficulty. We find that different investigators tend to discover widely different mechanisms of thought as lying at the root of the neurosis. The universality of the sexual basis so vehemently asserted by partisans is strenuously denied by other investigators, who them-

selves propound mechanisms that are not admitted universally. On any theory all these observers cannot be right, and in physical science, if we found that honest experimenters obtained consistently different results with a certain method we should not hesitate to impugn the validity of the method. A form of inquiry which involves the exclusive concentration of the mental activity of the subject on the performance of a task set to him by the observer renders him particularly sensitive to any form of stimulus that he may receive from the observer. We have seen in the preceding lectures that the expression of mental activity is far from confined to vocalisation. Movements of every variety are involved; the more obvious movements to an observer have in point of fact not been dwelt upon, that is, such movements as are evidenced by facial expression, eye movements, and the play of the pupils, for the sole reason that though obvious to the eye they present overwhelming difficulties to instrumental registration. An example from the animal world may illustrate the part that such movements play in communicating our approval or disapproval.

The Thinking Horses of Eberfeld.

I know of no better example of the perils that beset this form of investigation than the story of the "thinking horses" of Eberfeld. A Prussian schoolmaster, Herr Krall, succeeded in educating horses so that they would indicate the sum or product of numbers written on the blackboard by stamping the hoof the proper number of times. Later on they acquired the power of doing divisions and extracting cube roots. They were investigated by most of the eminent psychologists of Germany and many were the papers written about their feats. The bona-fides of their master were undoubted and they would perform equally well in his absence. As time went on these animals developed superhuman powers: they would not only solve the difficult problems of the blackboard, but they would guess correctly and stamp out the number of which the observer was thinking. At this stage they were investigated by Stumpf; he confirmed their marvellous powers of guessing the numbers thought of, and then proceeded to test their ability when blindfolded. The horse did everything it could to see the observer from under the bandage; when it succeeded in doing so the answer was right, but when the blindfold was effective it was generally wrong. That solved the mystery; Stumpf found that, in common with every observer, when the number that he had thought of was stamped out he made a little involuntary gesture of assent which the quick eyes of the horse noted. I have often repeated the same experiment, taking the part of the horse myself, and so long as the subject is innocent of the method employed I have seldom failed. To an experimenter conducting an analytic examination no stimulus word can be totally without affective import, be it the expectancy of a result or the recognition of its utility.

Responsiveness of the Subject of Inquiry.

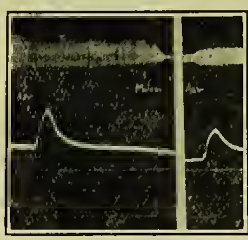
The subject is dissociated from all other stimuli than those communicated by the observer. He is warned to respond without self-criticism or hesitation. If with a casual fixation of attention it is easy to repeat the experiment of the Eberfeld horse under the conditions of a psycho-analytical investigation there must be many signs such as the inflexions of the voice and the play of the features, that it is impossible to control. In such a responsive attitude on the part of the subject wilful deception of course plays no part. Evidence of the extent to which the responsiveness of the subject can be raised under appropriate conditions can be found in the Proceedings of the Society for Psychical Research. Whatever be the eschatological value of these documents they certainly contain a mine of information to the student of abnormal psychology. The alertness with which almost imperceptible hints are adopted, the amazing accuracy with which the observer's mind is interpreted by perfectly honest subjects who have placed themselves as purely receptive instruments in the hands of the observer whilst under the belief that they are giving utterance to the thoughts possessing their brain, must give pause to anyone who notes the conditions under which analysis is carried on. Even when suggestion is discounted it is impossible to ignore the part played by the analyst in the interpretation of the results. I have sufficiently dealt in a previous lecture with the incommunicable uniqueness of states of feeling. It is for these and similar reasons that, in so far as it has appeared to be desirable to inquire into the associations connected with any particular phase of conduct, I have confined myself to a very superficial examination that readily permits of an objective control. The patient is allowed to tell his history without interruption or question whilst a continuous record of his skin resistance to a constant current is photographically recorded on a roll of bromide paper. The observer sits behind him, and the main headings of his discourse are taken down on a sheet of paper ruled so as to allow the time at which any statement was made to be compared with the timed record of his resistance. I have made many such records and they form curious psychological documents.

Inconstant Reaction of the Vegetative System.

It is by no means those statements to which the patient appears to attach most importance that furnish evidence of an affective response ; obvious attempts to achieve pathos may leave the galvanometer unmoved, whilst the narration of some less prominent incidents which reveal the general orientation of the patient's attitude will evoke a well-marked galvanic reflex. Even with such precautions the results obtained must be interpreted with considerable caution. Sherrington and Grünbaum have shown that stimulation of a cortical area after an interval by no means invariably evokes the same form of response, and we may interpret this as a physiological

demonstration of the fact that our reaction at any given time to a stimulus is determined to a great extent as to its form by the particular equilibrium in which the stimulus finds the nervous system. Still more so is this true of the mechanism of the affective response. The vegetative nervous system does not always react in the same fashion to the same type and intensity of stimulus. I was able in conjunction with Symes to furnish an experimental demonstration of this fact in connexion with the sympathetic innervation of the bronchial muscles. When the neuro-muscular junction of the sympathetic nerve-supply to the bronchiolar muscles is stimulated by the same dose of adrenalin a precisely opposite type of response may be elicited, according to the condition in which the stimulus finds the bronchiolar mechanism. If the bronchioles be constricted the stimulus will cause dilation; on the contrary, when the bronchioles are dilated sympathetic stimulation will cause constriction (Fig. 3). A very little observation of our own feelings reveals a similar process. A stimulus that has evoked a pleasurable feeling may on repetition prove disagreeable, and vice versa: "Nessun maggior dolore che ricordasi dei tempi felici nella miseria." It by no means follows that a stimulus evoking a galvanic response indicating its "nocuous" nature always did so. As a method of therapeutics the attempt to disintegrate the aberrant mechanism by the introduction of fresh associations, if the contention as to the organic disturbance underlying the neuroses be correct, can only claim to be a form of symptomatic treatment. In the absence of efficient radical treatment symptomatic treatment is justified, so long as it is not mistaken for a process of radical cure. It is as symptomatic treatment that the methods of psychotherapy must justify themselves, and there can be no greater disservice rendered to its claims than the talk of "cures" evidenced by the sublimation or removal of certain symptoms of a particular neurosis.

Fig. 3.



Antagonistic action of adrenalin on bronchioles. Firstly, when dilated; secondly, when constricted.

Hysteria.

So far I have dealt with the fundamental characters of neurosis in which the physical disturbance is manifested by a hyperactivity of the mechanism of affection. I will now turn to a type of neurosis in which I have reason to think that the fundamental disorder is an impairment of the activity of the mechanism of affection; the forms of neurosis that fall under this definition are those designated as hysterical or pithiatic. The conception of the nature

of hysteria has undergone a radical change in recent years. The supposed stigmata that seemed to constitute the neurosis an organic disease have been discarded one by one. Under the ægis of Babinski we have learnt to eliminate all symptoms from the clinical picture of the neurosis that are not entirely under the control of the will, until we have reached a point when our knowledge of hysteria may be summed up in Babinski's famous dictum—" Entre l'hystérie et la fraude il n'y a qu'une difference d'ordre morale." Hysteria, then, can only be distinguished from conscious simulation by a purely subjective criterion ; it may appear to be a retrograde step to return to the old search for organic disturbance after our symptomatology has been carefully purged of the so-called stigmata, which have one after another been shown to be simply the response of the patient to auto- and hetero-suggestion. If, however, the view that a neurosis is the expression of an organic disturbance is to be consistently maintained, an attempt to demonstrate the existence of signs of such disturbance in hysteria is a logical necessity. The implications of the view propounded by Babinski do not seem to have been clearly recognised. Starting with a purely objective analysis of conduct, he finishes by abandoning all attempts at an explanation in terms of neuropathology and refers us to a purely subjective standpoint. Any attempt to understand hysteria must be founded on some knowledge of the general orientation of the behaviour of the hysteric before his conduct brings him into contact with the physician. Such knowledge is rarely directly available to the specialist ; it may more often be gleaned from information supplied by the family doctor or by intelligent relatives. The picture obtained from these sources is that of an egocentric individual without strong or durable emotions, though very anxious to impress the outer world with the gravity and intensity of his experiences. Always more ready than a well-bred person should be to impress his fellows, he will not willingly apply himself to any task involving strenuous exertion, fatigue, or danger. The total personality conveys an impression of flimsiness. He appears to be a very different person to the volcano of ill-suppressed sexual passions that some writers on psychotherapy have portrayed. The symptoms of hysteria convey to the observer a certain general impression of what can be called theatricality—though possibly the stage ill deserves such a comparison. The necessity under which the cinematograph actor finds himself of conveying his mental states to the audience by emphasising all the mechanisms of expression has much in common with the conduct of the hysteric. Such vague impressions might lead us to look for indications of enfeeblement of the mechanism of affection which reinforces and protects the general tendencies of normal conduct. In 1918 I had the opportunity of examining all the cases of neurosis that passed through the Maudsley Neurological Clearing Hospital, which at that time had over six hundred beds. I found that the large class of patients exhibiting those motor and physical symptoms which

constitute the hysterical syndrome could be readily differentiated from other types of neurosis by the extraordinary depression of the galvanic response to any form of stimulus. Such patients would start or tremble at a loud noise or painful stimulus, when a normal man would hardly budge, but the galvanic response would be either absent or less than normal. Though they might talk of great emotional perturbation there was no corresponding galvanic reflex. The *mise en scène* of the galvanometer room seemed to be extremely conducive to thc exhibition of hysterical crises, and I had the opportunity of observing the galvanic reflex during many such scenes. One young soldier suffering from a hysterical contracture of the foot broke down during an examination ; tears rolled down his cheeks, he addressed his dead brother in language savouring of a South London mclodrama, he asked why he himself had not been killed in his brother's place so that the favourite son might have been left to comfort his poor old father, and all the time whilst he wailed and wept the spot of light from the galvanometer mirror remained steady. In my second lecture I pointed out that in the recitation of dramatic poetry the counterfeiting of rage or grief, no matter how dramatically performed, is unaccompanied by the electrical signs of activity of the affective mechanism. In these observations we have, I think, the key to hysterical behaviour. It is purely imitative. I published these results in a very condensed form at the meeting of the British Medical Association in the autumn of 1918, and since then have seen no reason to modify the conclusions that I then formulated. I not only found that the response to the alleged emotional states is absent but that the hysteric reacts subnormally to all forms of adequate, that is nocuous, stimuli. We are therefore entitled to assume that the activity of his mechanism of affection is subnormal. In the second lecture I demonstrated that a study of the time relations of the galvanic and other forms of response to nocuous stimuli shows that the stimulus is recognised and elicits an appropriate cerebral response an appreciable time before the bodily mechanism of affection reacts. We have, in fact, a kind of dualism—on the one hand the discriminatory mechanism associated with intelligent behaviour, and on the other the reaction of the organism as a whole to a nocuous stimulus. Herein lies the explanation of hysterical conduct. The hysteric is as capable as a normal man of recognising that the situation in which he finds himself is one of potential peril or discomfort, he can respond immediately to the situation by an appropriate cortical reaction ; but if the stimulus be continued his activity lacks the reinforcement supplied by bodily reaction. To determine his line of conduct a representation of the situation is necessary, and such a representation must normally involve the activity of the mechanism of affection, since in no other way can the unpleasantness of the situation which reinforces the intellectual appreciation of its danger be recognised. Deprived of this determining force his reaction to the situation will be at the mercy of any casual stimulus. We have had objective evidence that the affective reaction fails

him ; he has then recourse to other forms of expression more or less distantly associated with the feeling of unpleasantness. The association of bodily ailments with the feeling of distress will most readily furnish the mechanism by which the patient endeavours to represent to himself and to symbolise to the external world the fact that his activities are impeded or threatened by some nocuous influence in the environment. Hence the hysterical symptoms really constitute a method of expression primarily for egoistic and secondarily for social needs which has been conditioned by an organic disability of the mechanism of affection. Another cardinal symptom of the hysteric is referable to the same organic disability. All writers on hysteria have emphasised the abnormal suggestibility of the hysterical subject. Suggestibility has been defined by McDougall as a process of communication resulting in the acceptance with conviction of the communicated proposition in the absence of logically adequate grounds for its acceptance.

The destructive criticism of logic by the pragmatists has taught us one thing—that is, that no judgment is without its affective side, a truth which, though by no means new, since it is implicit in the revolt against formal logic which began in the psychological studies of Fries and Herbart, is still occasionally forgotten. Our strongest bond with reality is that of the feelings which constitute our strongest defence against the irrational. Mind dissociated from feeling is mind very much at the mercy of any suggestion. It is the bodily reaction against a suggestion that is in discord with the general tendencies of our activity that is the real guarantee against its acceptance. It is just this defence that the hysteric lacks ; to a greater or less extent his activities can be unduly influenced both by autogenous and heterogeneous suggestions. In other words, the innate and habitual pattern reflexes, having lost the mechanism of bodily reaction by which they reply to stimuli tending to interfere with their activities, are no longer the potent protective system that they constitute for the normal man and can be displaced or dissociated by any new stimulus of sufficient potency. Great as has been the work of Babinski and his school in demonstrating the mimetic nature of hysterical symptoms, it has been needlessly impaired by the criterion that they have adopted of what constitutes a hysterical symptom. Limited by the subjectivism that defines hysteria as only differing from fraud in its moral aspect, they decline to consider as hysterical any symptom that cannot be produced by an effort of will. This is a return to the subjective method par excellence. Of will and its limitations we have no objective knowledge, and as Spinoza once said : " No one has yet learned from experience what the body regarded merely as a body is able to do in accordance with its own natural laws, or what it cannot do. For no one knows enough about the constitution of the body to examine all its functions."

The study that we have just made of the movements manifested during cerebral activity has revealed a

series of motor, circulatory, and glandular reactions, which, though not capable of being directly influenced by volition, are in their totality the expressions of volitional effort. Owing to his dissociation from states of feeling the hysteric appears to readily assume that state of muscular hypertonicity which we have observed as an accompaniment of concentrated effort, and the reflex response to certain stimuli is in consequence greatly exalted. The response to auditory stimuli frequently takes the form of an exaggeration of the normal " start." I have pointed out that the latent period of the start in man is extremely short, not much longer than the 0·05 second found by Sherrington in the decerebrate animal and the exaggerated start of the hysteric, having the same latent period as that of a normal man, a voluntary origin must therefore be ruled out. Even more striking is the exaggeration of the knee-jerks. The response may not only be increased but followed by a series of irregular clonic contractions—which may, however, be readily differentiated from the true quadriceps clonus of a lesion of the pyramidal tract when recorded by the oscillograph or the string galvanometer (Figs. 4). As Wertheim Salomonson has pointed out, the electromyogram of the true clonus points to an origin different from that of the spurious hysterical clonus. None of these phenomena are directly referable to voluntary activity, but they are consistent with the supposition that the patient is in the same state of muscular hypertension as a normal person whose whole organism is concentrated on a particular voluntary effort. I have occasionally succeeded in obtaining myograms exhibiting a similar functional clonus from normal subjects concentrating all their attention on squeezing the dynamometer. It is generally accepted that when a case of hysteria is characterised by some paralytic or motor symptom which simulates the effects of a definite organic disturbance it is always possible to differentiate between the hysterical symptom and the organic one by the fact that such subsidiary mechanisms as would be certainly affected in organic lesions are not affected in the case of hysteria. In hysteria, owing to the ignorance of the subject of the physiology of the symptom mimicked, the affection of these subsidiary mechanisms would never be suggested to him.

Whilst in the great majority of cases this rule holds good, and is indeed sufficiently constant to be our standard method of differentiation between organic and functional disease, it is not quite universally true. It would appear that certain mechanisms are so closely allied with the normal exercise of particular functions that although their existence and connexions are unsuspected by the patient it is impossible for the main function to be suspended without their being also affected. Hurst has pointed out that in cases of total hysterical deafness the start or jump to a loud sound may be suppressed. Such suppression of an involuntary mechanism does not, however, appear to be true of the affective reactions. I have never failed to obtain a galvanic reflex to

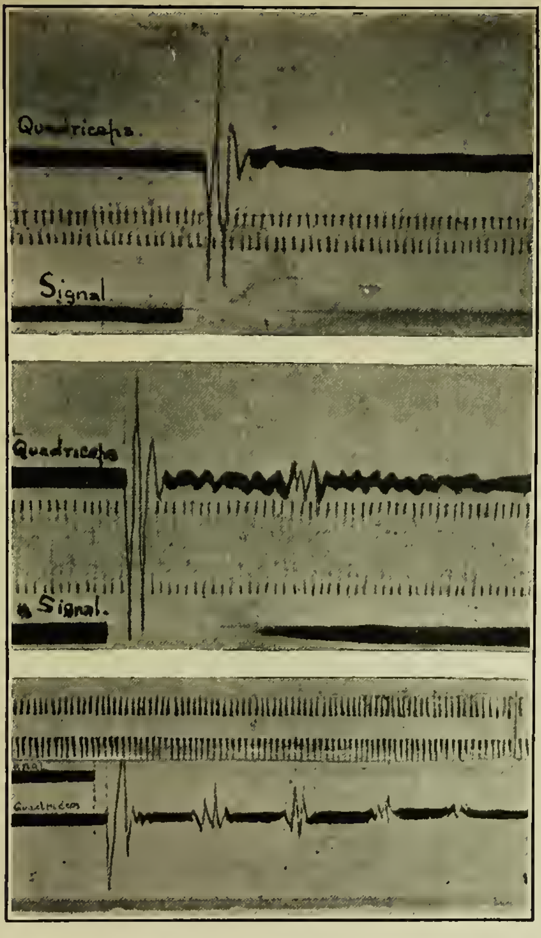
a loud sound in cases of hysterical deafness. Another example of the difficulty in drawing an absolute line between voluntary and involuntary symptoms in hysteria is furnished by the vestibular reactions.

FIG. 4.

A

B

C



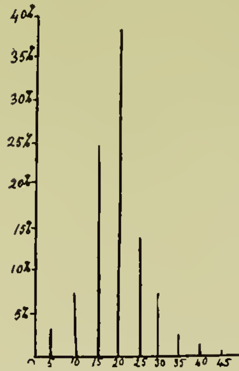
Electromyograms taken with Bock-Thoma oscillograph.

A, Normal knee-jerk. B, Hysterical knee-jerk, with functional patellar clonus. C, Knee-jerk, with patellar clonus in spastic paraplegia.

I have been able successfully to suggest a definite vertigo to two cases of hysteria who complained of ill-defined symptoms of giddiness. In both these cases when they were asked to perform the pointing

test with their eyes shut they invariably showed a deviation corresponding to the direction of the suggested vertigo. Thus in the absence of the start reaction we have an involuntary mechanism definitely suppressed and in the vertigo reaction we have it appropriately altered, when a strict interpretation of hysteria in the terms of Babinski's definition would require them to remain unaffected. What may be the nature of the pathological changes responsible for this hypoexcitability of the mechanism of affection in hysteria it is impossible at present to specify. That the organic change exists prior to the development of the more obvious symptoms may be inferred from a study of the previous history of such cases. The diagram (Fig. 5) is constructed from the statistics of Kraepelin of the age-incidence of hysteria based on 430 cases. Although the years immediately succeeding puberty are those most affected by "Sturm und Drang" it would hardly be expected that the fall in the curve would be so steep were it not that a definite amelioration of the organic response rather than a disappearance of environmental stress is the responsible factor.

FIG. 5.



Kraepelin's curve of age-incidence in hysteria.

CONCLUSION.

At the risk of wearying by reiteration, I will again call your attention to the absence of any proof that the early experiences of the hysteric differ from those of the majority of mankind and to the fact that they therefore cannot be held to play any considerable part as predisposing factors. Of the disturbances of mechanism responsible for the psychoses I do not propose to speak. We have already seen how the observations of Mott and his pupils may be considered to have definitely settled the question of the organic origin of dementia præcox. The profound metabolic disturbances in manic depressive insanity, though still awaiting investigation, are indicative of a similar organic basis. Through the kindness of Dr. Mapother I was able to satisfy myself that the seemingly violent emotional activity of mania is unaccompanied by objective signs of hyperexcitability of the mechanism of affection. Were but a tithe of the attention at present devoted to the classification and tracing of psychogenic disturbances in the insane given to an objective study of their nervous mechanism by physiological methods, we should probably before long have remedied the deplorable condition of the pathology of insanity. Unhappily asylum authorities have not yet recognised the necessity for observers skilled in physiological methods.

In concluding these lectures I must acknowledge with the profoundest sense of obligation the kindness and encouragement in their preparation that I have received from Sir Frederick Mott, in whose laboratory at the Maudsley Hospital the experimental work connected with them has been performed.

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